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XUM

THE

JOURNAL OF SCIENCE.

NOVEMBER, 1881.

I. MINIATURE PHYSICAL GEOLOGY.

By C. LLOYD MORGAN, F.G.S., Assoc. R.S.M.

I.

SOME four years ago I described in the columns of "Nature" a few observations I had made, during seaside walks, on the subject of Miniature Physical Geology. The observations were such as might be made by anyone at almost any time. Still I thought that lessons were to be learnt in this way. And I ventured to draw attention to some of these lessons. I concluded my communication to "Nature" with these words:—"My object in drawing attention to such matters of ordinary observation is to induce young students of physical geology to go out and observe these things for themselves. If, after a morning's study of Lyell's 'Principles,' the young geologist will devote an hour's careful observation to miniature physical geology, with sketch- and note-book in hand, he will find that his conceptions have a reality and a solidity which could not have been evolved in the study at home, while at the same time he will find it more easy to grasp, when he shall have the opportunity, the workings of Nature on a grander scale." In this paper I propose to treat this subject a little more fully, to describe shortly such simple observations as I have made, and to point out how frequently we may see for ourselves, during a seaside or hillside ramble, representations on a small scale of some of the grandest works of Nature.

There are few of us, I imagine, that have not felt the advantages of pictorial illustrations as aids to comprehension. But in some subjects working models are more

valuable aids than pictures. Now I look upon a little streamlet as a working model, supplied by Nature herself, to illustrate the actions of great rivers. And I would ask my readers to look upon all the observations I am about to bring before their notice as small-scale exemplifications of the ways of Nature. Taken by themselves they are trivial to a degree. Taken in connection with the phenomena they exemplify, they may not perhaps be without interest.

II. *The Catchment Basin of Rivers.*

On almost any flat sandy shore one may watch the gradual establishment of miniature drainage areas and catchment basins. On one occasion, when I had been collecting fossils from the tertiary beds at Pegwell Bay, in Kent, I was caught, as I crossed the smooth flat left by the receding tide, in a heavy thunder-shower. To my eye the surface seemed perfectly even; but as I stood and watched, the rain-drops collected into definite channels: these soon formed miniature rills, uniting into miniature streams, which ere long in their turn united into a miniature river. This kind of illustration is, however, so well known that I should not have drawn attention to it here had I not been able, in less than an hour and a half, to watch the separation of a mud-surface, to all appearances quite even, into a number of separate and distinct drainage areas, each of which resembled a model of the catchment basin of some great river.

III. *The Cutting Sideways of Rivers.*

On almost any stretch of sand the action of a miniature river on its banks may be watched. These little streams seldom or never flow in a straight course. I have sometimes on a sandy shore done my best to make a straight run for a little stream, and then, damming up its bed, deflected the miniature river into the artificial canal I had thus constructed. For a little while the stream would continue to keep a straightforward course; but very soon some slight obstacle on one side would throw the main current towards the opposite bank. The result of this is soon apparent. This bank recedes, and, as it does so, it ceases to be a shelving *slope*, and becomes a tiny *cliff*. It continues to be rapidly undermined by the action of the stream, and the upper portions now and again topple over with a little splash into the water. In this way a bold curve is formed, which *increases in length down stream*. In the meanwhile, on the opposite shore of the

river, sand is deposited, and as the river cuts its way downwards this portion is left high and dry.

But the mischief once begun rapidly spreads. The formation of one such curve sets the stream a swinging through the rest of its course. Now on one side, now on the other, are the banks eaten into, until the course has become marked by sinuous curves, local conditions causing some of the curves to be more pronounced than others. All the curves, however, have a tendency to *advance down stream*. When a curve has in this way become comparatively long, the miniature river often tends to assume a straighter course; it recedes from its bank cliffs, and soon a tract of comparatively level dry land separates these banks from the stream. This is generally due to the lengthening of the curves above. In fact, a little consideration will show that the gradual advance of the curves down stream will have a tendency to produce a broad valley through which the river will wind. These miniature valleys have often beautifully-marked river terraces on either side, showing the length of swing of the river on each occasion that it oscillates to and fro. There can be little doubt that during each swing the river cuts its way downwards to some extent, and that therefore the terraces formed in succession on the left and right banks are at slightly different levels in a descending series; but this I have not been able to see in miniature.

How marked a feature broad valleys and river terraces are on a large scale I need hardly remind the readers of this Journal. Memphis, on the Mississippi, stands on such a terrace. Sections of any tolerably large river-valley—that of the Thames, for example—show this well, but to make out a succession of these terraces in a large river valley is not easy. They may be more satisfactorily studied in smaller tributary streams. I remember an admirable instance near Penrith, in Cumberland. But for my own part I confess that the study of miniature valleys helped me much in the comprehension of this subject.

Very plain are the evidences of the swing of the stream in great rivers. No one who has passed down the Mississippi, for example, can have failed to notice that the opposite banks are generally very different in character. The one shelves down into shallow water; the other is cliff-like, often 20 feet high or more, and has deep water under it. The shelving bank is in fact advancing on the river. The steep bank is being undermined by the river. All the settlements on the banks of the Mississippi are on the steep side; there are none on the shelving bank. The reason of this is

obvious. Steamers can come alongside the steep bank; they cannot get near the other. In any case, however, such settlements cannot be permanent. If they were built on the shelving bank the river would be constantly receding from them, and they would have to follow it. As it is they have to recede before the advancing river. I was informed at one place (Point Pleasant), on the Mississippi, that the river had advanced nearly three miles in sixty years. In 1876 I came across an instructive example of this kind of action. When I was in Lancashire a gentleman showed me a map of his estate made in 1747, in which year the River Lune ran, if I remember right, within a hundred yards of his house. In 1876 the river was fully one-third of a mile distant, and a river-terrace about 50 yards farther off marked the limit of the swing of the stream in that direction. As the river forms the boundary between two properties, belonging say to A and B, one gains while the other loses, as the channel shifts. In this case A gained and B lost; whereupon B put up a stone groin, to throw the force of the stream on A's bank, that he might regain some lost ground. A in return put up a similar groin, to catch the brunt of the stream and turn it back upon the opposite bank. When I was there I saw several such artificial constructions, which will probably have the effect of checking the swing of the stream.

On the Mississippi I have seen great masses of the steep bank fall, and felt in a large steamer the wave produced. Mr. Bates, however, has a striking passage describing similar falls on the Amazons. "We saw the work of destruction," he says, "going forward on the other side of the river, about 3 miles off. Large masses of forest, including trees of colossal size, probably 200 feet in height, were rocking to and fro, and falling headlong one after another into the water. It was a grand sight: each downfall created a cloud of spray; the concussion in one place causing other masses to give way a long distance from it, and thus the crashes continued swaying to and fro with little prospect of a termination."

IV. *The Formation of a Cut-off.*

Under certain circumstances the action of a river upon its banks produces a curious effect. This I have seen in miniature on one occasion only. On the muddy flats of Pegwell Bay a little stream bent round in a great loop something like the capital Omega in the Greek alphabet. The flow of the water past the curves caused the concave banks to recede, thus narrowing the neck of land which separated them.

This continued until the isthmus of land was eaten through, whereupon there was at once a flow of water through this opening, which was in consequence soon broadened and deepened to such an extent as to admit of the passage of the whole river through this channel. The loop was thus left as a horse-shoe lake in connection with the river.

Those who have passed down the Mississippi from St. Louis to New Orleans will doubtless be able to recollect passing through more than one "cut-off" of this nature, and thus shortening the journey by perhaps eight or ten miles. An instance of the same thing, which took place on the Juruá, a Brazilian river, is described by Brown and Lidstone in the following sentence:—"Within the last six years the river has altered its course, by cutting through a neck of land, thereby taking a short cut and abandoning a long curve." The upper end of this curve was subsequently silted up.

I remember well, as a boy, anxiously watching the narrowing of the isthmus separating the two concave banks of such a loop on the Wey, a tributary of the Thames. And I have a vivid recollection of the occasion of my first "shooting" in my canoe the rapid which was formed when the bank had at last given way.

V. *The Formation of Islands.*

To revert now to the miniature river: in the midst of the stream sand-islands are from time to time formed, often caused by a stranded stick, and partly no doubt by the deepening of the main channel on one side or the other; but no sooner has the sand of which they are composed become dry than the treacherous stream commences the destruction of that which it has itself produced. This it generally begins at the upper end, and it is not uncommon to see construction and destruction going on at the same time; destruction above, construction below. Where this double action takes place the island is not stationary, but in motion down stream.

Whether this motion down stream has been observed on a large scale I do not know; but that the construction and destruction of islands take place with considerable rapidity in some of the larger river channels there can be no doubt. In one of my note-books I find the following remarks, written on board a Mississippi steamboat:—"You can see islands in all stages of formation in the river. First, an extensive shallow or shoal; against the upper portion of this logs and

snags get stranded, and these protect the rest from the force of the current. In the next stage the sand-banks are higher, having probably been built up during a flood. Then a few cotton-wood shrubs, the growth of which is very rapid, are seen here and there. These in two or three years cover the island, and attain a height of 14 or 15 feet. Beneath them springs up a considerable undergrowth of cane. The island is now complete, and ready to be ere long washed away by some change in the direction of the current." I find, too, another note written on the Parà branch of the Amazons:—"At Parà the river is, I am informed, 9 miles wide, and is studded with many islands. Not unfrequently some of these islands are washed away, and fresh ones formed. For instance, a few years ago, Parroquet Island stood some few miles below the town: this has disappeared, and on the other side of the stream a new island,—the Ihla Nova,—has arisen and is covered with a luxuriant vegetation."

VI. *Changes of Depth in River Channels.—Sand-dragging.*

As illustrating the varying depth of the channel in large rivers, the following observation, made on the sands near Ramsgate, is perhaps not uninteresting:—The stream had to pass through some narrows between a miniature chalk cliff and some other obstruction. The force of the current here dug a hole in mid-stream. This hole gradually lengthened down stream. Then a little ridge began to separate it into two parts, an upper and a lower. The lower depression advanced, and was followed by the ridge. By this means the upper depression was lengthened, until it was also separated into an upper and lower part by a ridge. In this way several depressions in the bed of the stream were formed. These advanced down stream, and the lowest of them was some considerable distance from the narrows, but below this point the depressions were so far filled in as to be indistinguishable. This is a good instance of the way in which fine and coarse sand is *dragged* along the bottom. Of course in the case of a large and deep river this action would not be so noticeable; but I imagine that the estimate of Messrs. Humphreys and Abbot, that the Mississippi pushes along the bottom sand and gravel equal to about one-tenth of the mud held in suspension by the river, is by no means excessive.

VII. *The Cutting Downwards of Rivers.*

The best miniature examples I have seen of rivers eroding for themselves deep channels, by cutting their way downwards, were near the Barton cliffs, in Hampshire. But almost anywhere one may notice this general deepening of channel giving rise to a valley or ravine. There was, however a special interest in one example I saw at Barton, which induces me to mention it here. At the base of the cliffs was an eroded surface of Barton clay which was comparatively tenacious. But the surface was not even, the hollows being filled up with a less tenacious mixture of mud and sand. Across the whole of this flowed a little freshwater stream, which had cut its way 6 or 7 inches into the surface. And it was interesting to notice that where the clay was tenacious the valley sides were steep, almost precipitous; where the stream passed over the mixture of clay and sand, the sides were sloping at a very gentle angle. This gentle angle could not, however, I think, have been produced by the stream alone, but must have been the result of either rainfall or the spray from the waves at high tide.

Those who have seen anything of river action on a large scale will have noticed how marked an effect the hardness of the rocks has upon the form of a river valley. Where the rocks are soft the valley is very broad and open; where the rocks are hard the valley assumes the character of a gorge. Of this the rivers which take their origin in the Weald, and flow through the South Downs, afford well-known examples. The Rhine gives us another illustration. And if we read the "Geological Report of the American Survey of the Colorado of the West," we find a description of the same thing:—"Where we passed through the first of these ranges (the Purple Hills) it has nearly a N.W. and S.E. trend, and in the immediate vicinity of the river is composed of a gray massive granite, which, yielding somewhat readily to the action of the elements, has formed slopes receding from the river, giving the pass an outline strikingly in contrast with that of most of the cañons cut in the porphyrite rocks higher up" (p. 21). One of these ravines, the great cañon, as is well known, has nearly perpendicular walls, which are in one place more than a mile high. Nevertheless the American geologists ascribe its formation entirely to the action of running water.

An instructive small scale example of a river thus cutting

its way downwards may be seen on the Cape Flats, not far from Cape Town. Near the Diocesan College, Rondebosch, runs a small stream, known, I believe, as the Little Black River. As I walked along the dry bed of the stream I found my path continually obstructed by roots, which run right across the channel from one bank into the other, and are at a height varying from ½ a foot to 2 feet 6 inches from the present bed of the stream. It is clear that these roots cannot have got across from one bank to another as the channel now is. They must have passed beneath the former bed of the stream, and they now remain to show how rapidly the stream is cutting its way downwards.

(To be continued.)

II. LATENT HEAT.

By CHARLES MORRIS.

(Concluded from page 533.)

A QUESTION now arises as to the efficacy of this process of equilibration of temperature. In truth the differences of sensible heat between the orbs and the matter of space are being but very slowly rectified, if at all. The principal, and perhaps the only, heat emissions from the orbs into space, are in the form of radiant heat. This is a surface emission from the spheres, and it possibly never becomes static heat until it reaches the surfaces of other spheres. The vast volumes of motive energy thus poured into space may pass through a completely transparent medium, and, if so, can have no effect in raising the temperature of interspheral matter. The spheres tend, in this manner, to produce temperature equilibrium among themselves, but may fail to affect the matter of space.

The radiated energy is superficial only, and must depend for its supply upon a very slow conduction of internal heat to the surface, or on a more rapid convection while spheres continue liquid or gaseous. In one or the other of these modes the internal heat of spheres reaches their surfaces, and is thence radiated outwards. There is probably no heat conduction through the atmosphere. There is a certain

degree of convection, but there is no good reason to believe that sensible heat is transferred to interspheral matter in this mode. And as radiated heat may not affect the temperature of the matter of space, it is quite possible that the diversity in temperature is not being, and cannot be, rectified.

But heat exchange between the spheres is active. Every touch of a solar ray on a planet conveys heat energy from the sun to the planet. Thus the inequality of sensible heat between the orbs of space is being slowly overcome. This inequality is not alone a result of the more rapid cooling of the smaller orbs, but arises largely from the differences in gravitative vigour of the orbs. Gravity on the sun is much greater than on the earth. Compression of the internal matter of the sun must greatly exceed that of the earth. Consequently the specific heat of solar substance must decrease, and its latent heat become sensible, to a much greater extent than in the earth. In consequence of this the earth could never have reached the temperature of the sun, and possibly its highest temperature was far below the present solar temperature. In like manner the highest degree of lunar temperature must have been considerably less than that of the earth. This is, doubtless, one main cause of the heat inequality between the larger and smaller orbs, the greater rapidity of cooling in the latter being the other cause. And from this results the long process of equilibration to which all existing planetary activity is due.

Yet it must not be supposed that when equilibrium of temperature is finally attained the spheres will cease to emit heat. For radiation from the spheres is not to other spheres, but to the matter of space, and the falling of these rays on other spheres is, in a certain sense, accidental. When, however, temperature equilibrium is established between the spheres, thenceforward each will absorb as much radiant heat as it emits, and the radiations of heat into space will be just balanced by the receptions of heat from space. Yet this final equilibrium of heat emission will not be an equilibrium of temperature. If we take the sun and the earth as illustrations, we find the earth receiving heat from the whole hemisphere of the sun, and returning heat only from its own much smaller hemisphere. The area of the sun's hemisphere is 12,000 times that of the earth, and if the temperatures of each were equal, and each radiating heat with equal energy to the other, the total heat received by the earth from the sun must considerably exceed that received by the sun from the earth. Necessarily, then, the surface temperature of the sun must

fall considerably below that of the earth before their heat emissions can be in equilibrium. The same rule, of course, applies to all spheres which exchange heat with each other, and thus a final equilibrium of heat exchanges could not be an equality of temperature in the orbs of space.

Of the heat emitted by the sun only the 227 millionth part is caught by the planets, and this is nearly all yielded again to space. The vast remainder of the solar radiations flows out towards the remote spheres of the universe. Each of these receives but a minute fraction of this heat. Whether it shall be all eventually absorbed or reflected depends upon the improbable contingency that every possible straight line drawn outward from the sun shall somewhere meet a sun, a planet, or a smaller mass of condensed matter. It is the same with every other orb. Thus the heat radiated by the orbs into space can only in a minor sense be said to employ itself in producing temperature equilibrium between these orbs, since the great volume of it must wander unceasingly through space. Therefore space receives, in the radiant form, the excess temperature of the spheres, and is becoming, as it were, crowded with such ever-crossing rays.

But is the transparency of the matter of space to radiant motion perfect? May not its capacity for these rays be limited? A good electric conductor will transmit feeble currents with little or no resistance, yet will strongly resist powerful currents, and partly convert the electricity into heat. The same rule may apply to radiations, which may become more and more converted into static or sensible heat as the current of heat-rays becomes stronger. Crossing, or reversely moving rays, or parallel rays in different phases of vibration, may also partly obliterate each other, and yield static heat vibration. It is possible, therefore, that radiant heat may slowly become converted into static heat in space, in which case temperature equilibrium between all matter may be slowly arising. If it ever be completely established, then the assumed original equilibrium of temperature, and of absolute heat in nebular matter, will be succeeded by a final equilibrium of temperature and an extreme diversity in absolute heat, the latent heat of the rare matter of space being immensely greater than that of the dense matter of the spheres. The absolute heat-contents, or motive energy, of a molecule at the centre of a dense sphere, must be greatly less than that of a molecule in interstellar space to give them the same temperature resistance, the one being vigorously aided in its resistance by its fellows, the other having to depend solely on its individual energy, and the one

having to fight for the possession of a far smaller field of free space than the other.

It might be well to cease at this point, yet there are certain other interesting possibilities of the continued evolution of the universe, which may be briefly considered. Let us suppose that gravitation affects all matter, and that the rare matter of space is more condensed around the spheres than in interastral space. In such a case the density of this matter might greatly differ. But if all matter be in temperature equilibrium, the latent heat of the very rare interastral matter must considerably exceed that of the denser matter surrounding the spheres. But these spheres are rapidly plunging through space, and presumably leaving regions of condensed matter to enter regions of rare matter. A double result must follow. The condensed matter of the first region, being relieved from attractive pressure, must rapidly expand, and its temperature fall. The rare matter of the second region, being exposed to attractive pressure, must rapidly contract, and its temperature rise. In this manner a considerable disturbance of temperature equilibrium might result. And this disturbance would be augmented if chemical disintegration accompanied the first process, and chemical integration the second. Radiation would recommence from the heated to the chilled space, and from heated space to the spheres. And life, with all its powers, might arise from such an incessant disturbance of temperature equilibrium by the moving spheres.

But such an action could have but one result. A constant breaking away of the spheres from attracted matter must tend to decrease the motion of the spheres themselves, and eventually bring them to rest. And the heat activity produced in this process would virtually amount to a conversion of the energy of spheral motion into heat energy. It therefore could be but a temporary process.

Another result of such a frictional resistance must be the gradual yielding of the smaller spheres to the attraction of the larger, and a final aggregation of all spheres into a limited number of huge orbs. Only, in fact, after all mass motion had finally ceased, and gravitation had produced its utmost possible change in the distribution of matter, could a fixed equilibrium of temperature arise, and all motion be reduced to the heat vibration of separate particles.

This cosmical evolution, carried to this conceivable ultimatum, would consist in a gradual change from an original condition of partial homogeneity of distribution in matter, temperature, and absolute heat, to a final condition of homo-

geneity of temperature, but of great heterogeneity of distribution in matter and its absolute heat contents. And the sole cause of this long-continued change would be the attraction of gravitation. Could this attractive influence act sufficiently long upon the matter of any extensive region of space, without resistance from any reversing influence, the series of changes indicated in this paper might all take place. Nor would the series of changes end here. They could not cease until the utmost effect of gravitation had been produced.

If we consider the case of some great ultimate orb, at rest in space, what would be the final result of its attraction? Assuming that all substance is subject to attraction, the tendency would be to condense the matter of space around this orb. But such condensation must have its limit. It could not possibly continue until all matter was solidified, and space denuded of its contents. For it must be remembered that the effect of gravitation is really twofold. The pull on each separate particle is excessively slight, and diminishes rapidly outward from the solid surface. The weight of gas volumes is more largely a result of their compression by the gravity of atmospheric matter above them, than of their intrinsic attractive energy. But in remote space this compression must be inappreciably slight, and the attractive energy be so reduced that but little density could exist. Therefore, however intense the attractive force of the great final orb, it could not possibly reduce to solidity the matter of space. However dense the immediate atmospheric portions of this matter, the remote portions must continue in a state of excessive rarity, and but slightly affected by gravity.

Thus the final state must be an arrangement of matter in successive zones, varying gradually from the utmost density at the centre of the orb to the utmost rarity at the outer borders of its attractive control. Accompanying this would be a reverse distribution of heat, the capacity for heat being greatest in the zone of rarest matter, and least in the densest zone. At the centre of the orb heat energy might almost cease to exist, the motive vigour necessary to give this highly compressed matter a certain temperature being excessively less than that needed by very rare matter.

We may briefly glance at another possible result of the process of evolution here indicated. Chemical activity has accompanied it from its beginning, and may accompany it to its end, but the effects of chemism have become successively more and more complex. For a long period inor-

ganic chemism was very active upon the earth, and doubtless very energetic displays of motive force took place. Now inorganic chemical activity has almost ceased to exist, and organic chemism has taken its place. But to the full development of organic action violent and irregular displays of force are very adverse, and we find that organic life has advanced to higher and higher forms in proportion as the energy of inorganic action decreased. When complete regularity in material conditions and changes takes the place of the present irregularity, organic life may reach its highest development, while not only inorganic activity may cease to exist, but also the lower stages of organic activity. This seems the tendency of chemical change, and we have warrant, therefore, to look forward to a period in which all free energy will be exerted in the production of living forms, the only active inorganic agencies being those necessary to the production of vegetable forms.

For such a result it is not necessary that the kinds and conditions of matter, and degree of temperature, shall continue precisely as they exist at present upon the earth. Nature may have far more versatility in this respect than she is usually credited with. Thus in the production of terrestrial organic life just such materials are used as are most available, the gaseous carbon and oxygen, and the liquid water and ammonia. Of the solid constituents of the earth only those are employed which are widely diffused and readily soluble. Thus organic life employs here all the material which is in a condition adapting it to chemical action, and in quantities closely conformable to the chemical activity and availability of each element. And one element is frequently replaced by another when the latter becomes more available.

As it is possibly unimportant what materials take part in organic integration so that their physical condition is such as to adapt them to active chemical change, so it may be unimportant what the ruling temperature is, if it be only such as to preserve matter in this necessary physical condition, and to excite its chemical activity. On other spheres than the earth life may flourish under widely different conditions, both as to material and to temperature. And finally it is unimportant whence comes the energy employed in organic chemism. On the earth the energy of the solar beams is employed, this being the most available and vigorous source of motive energy. But elsewhere heat force arising from local sources might be employed; and if this energy were applied directly and wholly to the furtherance

of organic chemism, it would need to be far less in quantity than that of the sun, of whose total radiations to the earth only a minute fraction is thus employed. It seems quite possible, then, that organic life may exist elsewhere under conditions strikingly unlike those existing upon the earth, and may continue to exist and develop through great changes in the future conditions of matter.

Throughout this paper I have confined myself to the probable results in the case of attraction being the only force acting upon matter, or to the contingency of the material contents of some extensive region of space being enabled to display all the possible results of gravitation, without reverse influence from without. But if, as argued in my paper in the "Journal" of November, 1880, the amount of repulsive energy in matter just equals that of attractive energy, a reversal of all these changes must eventually occur, and every spheral condensation of matter be sometime succeeded by a nebular rarefaction of the same matter.

III. THE VACCINATION QUESTION RECONSIDERED.*

WE have exceedingly little sympathy with "movements," agitations, and societies for the abolition of this, that, or the other. Nor do we share Mr. Taylor's touching faith in "the common sense of our countrymen," or in "the force of truth when not suppressed." We hold that no people are so readily led astray by sophists and rhetoricians as are the English. At the same time we must avow ourselves agreeably disappointed with Mr. Taylor's pamphlet. Instead of sensational appeals to the emotions and prejudices of the ignorant, and of any misrepresentation of opponents, we find sober reasoning, candour, and courtesy greater than is sometimes shown by

* Vaccination: a Letter to Dr. W. B. Carpenter, C.B. By P. A. Taylor, M.P. London: Allen.

Has our Vaccination Degenerated? By C. Cameron, M.D., M.P. In "Fortnightly Review."

Dr. Carpenter in his controversial writings. Still, an undercurrent of dislike to the medical profession is not entirely wanting.

Further, the subject is not one of those which can only be rightly handled by men of special scientific attainments. Were such the case we should be the first to refuse outsiders the claim of being heard. But here is a certain phenomenon —the smallpox. Here also is a certain agency, *n*, which is asserted to put a stop to the occurrence of such phenomenon. If this assertion is correct, then in proportion as *n* becomes more general the smallpox must become rarer. Whether such is the case or not any educated man is able and entitled to judge, if the statistics of vaccination, and those of smallpox cases and smallpox mortality, are fairly laid before him. The matter is here somewhat complicated by a circumstance to which Mr. Taylor has very fairly drawn attention: smallpox, namely, is not—like, *e.g.*, phthisis—a constant disease which carries off year by year, or decennium by decennium, approximately the same number of victims. On the contrary, it makes its appearance in irregular epidemics. One year it may be almost absent, whilst the next it may assume quite alarming proportions. This fluctuating character gives, it must be conceded, great scope for a disloyal manipulation of statistics—a circumstance of which we fear the disputants on both sides have sometimes, though perhaps unwittingly, availed themselves.

We will open our enquiry by considering what grounds would justify an objection to the Compulsory Vaccination Act, and an agitation for its repeal. To us there appear two only as legitimate, viz., a want of efficiency and a want of safety, or, of course, the two combined. By "want of efficiency" we mean the failure of the operation to insure immunity from smallpox, and by "want of safety" we refer to the possible introduction of other—and worse—diseases. Of these grounds the latter, if demonstrable, is incomparably the graver. Indeed if vaccination, though less effective than its champions suppose, is at any rate harmless, the agitation against its legal enforcement is scarcely warrantable.

As to other than physiological grounds, we refuse to listen to them altogether. If the operation is a preventive of smallpox, and exposes those who submit to it to no other dangers, we hold that the State is bound to enforce vaccination upon its citizens. With those who think that the individual is at liberty to make his house a *nidus* of infection we can hold no discussion, because there is no common principle

to which both sides can appeal. Hence the "natural rights of parents" can be taken into account when, and only when, the uselessness and the perils of vaccination shall have been clearly shown.

It would be interesting to trace what was the original standpoint of the anti-vaccination movement. Did the recusants start with a conviction of the inutility and the risks of the supposed safeguard? Or did they take up originally a theological or political position, and have afterwards sought for medical evidence in its support? In the latter case we must hold the movement utterly irrational in its rise, though of course any sound arguments which may have been since brought forward are not invalidated by original error.

The anti-vaccinationists rightly lay great weight upon the possible communication of syphilis by the vaccine lymph. To this we may refer below. But we would ask, Are not some of their leading spirits also agitators against the Contagious Diseases Act? And if so, are they not guilty of a slight inconsistency in defending the existence of the dire affection whose communication they so much dread?

But we must now turn to Dr. Carpenter's eight propositions, the first seven of which assert, in slightly varied forms, the efficacy of vaccination, whilst the last seems to sneer at the existence—or at least the extent—of the danger of the simultaneous transmission of other diseases. Every one of these propositions Mr. Taylor traverses, not without ability, and to a certain extent successfully. If we do not misunderstand him, however, he maintains that vaccine is not, and never was, a protective—an evident exaggeration. It is not in our power to verify all his quotations, and to refer to the statistical returns he has made use of. But there are certain broad open facts which weigh in his favour. No one will dispute that in the first two quarters of the present century vaccination was not nearly so general as it is now. It was confined to what are commonly called the upper and middle classes, and even amongst these it was far from universal. Yet prior at least to 1838 a person with a pitted face was looked on as a curiosity—a survival from the days of ignorance. In our childhood we constantly heard the small-pox spoken of as an extinct evil. None of our schoolfellows or associates had been attacked by it, nor had, apparently, any of the children or young people whom we chanced to see in the streets. No one hinted at the necessity of re-vaccination. How a minority of perhaps 25 per cent—to take an outside estimate—could protect the non-vaccinated

majority was never asked. Since those days half a century has passed; vaccination has been made imperative, so that the "unprotected residuum" has shrunk to about 3 per cent. More than this: re-vaccination at adolescence is now insisted upon. This was the case in Prussia as early as 1836, as we know from personal experience. But now Dr. Austin Flint, writing in the "North American Review" for June, considers re-vaccination every five years as advisable. What a practical abandonment of Jenner's original doctrine! To return: in spite of this extended and repeated protection smallpox is openly and palpably more frequent now than it was in the earlier part of the century. Epidemic follows epidemic in quick succession, each, apparently, more rampant and more fatal than its predecessor. Thus the deaths from smallpox in London are given as—

1851—60	7150
1861—70	8347
1871—80	15543

Each decennium thus far appears worse than the one foregoing. What is still more striking, the smallpox extends to an ever-increasing proportion of those who have been duly vaccinated. Thus the official "Report of the Smallpox and Vaccination Hospital for 1866," as quoted by Mr. Taylor, makes the following alarming confessions:—"The ratio of vaccinated cases to the whole admissions of smallpox patients has gone on progressively increasing; thus—

Sixteen years ending 1851 ...	53·0	per cent.
Epidemic, 1851—2	66·7	,,
,, 1854—6	71·0	,,
,, 1859—60	78·0	,,
Years ending 1866	81·0	,, ,,

Dr. O. Cameron, M.P., whom Mr. Taylor calls "the recognised champion of vaccination in the House of Commons," states, in his article in the "Fortnightly," that the number of smallpox cases per million of vaccinated persons is greater now than in the beginning of the century, and that the death-rate in cases of smallpox after vaccination has risen from 1·75 per cent in 1819 to 9·2 per cent in the years 1870 to 1879, the increase of mortality being *most remarkable* in the *best vaccinated* class of cases.

The "Lancet" (June 18th, 1881) accepts "the recent smallpox epidemic in London, so far as regards the death of

children, as evidence of the failure of the vaccination system as at present carried out."

What do these facts mean? Mr. P. A. Taylor, if we do not misconstrue him, cuts the knot by declaring that vaccination never was a real factor in the question at all,—that its apparent success at the beginning of the century was due merely to its introduction coinciding with a temporary lull in the disease. Since then successive waves of the epidemic, more and more severe, have swept over us, and the impotence of the safeguard becomes each time more and more manifest.

Dr. Cameron, as we think, more impartially holds that vaccination had, and still has, a certain power, which, however, is gradually declining, owing to the transmission of the lymph through such multitudes of human systems. This degeneration, joined to a temporary exacerbation of the character of the disease, may well account for the present lamentable results. The saving-clause of the "Lancet"—"as at present carried out"—admits of more than one interpretation. It may amount to a charge of negligence brought against the surgeons who perform the operation. It may imply the notion conveyed in Dr. Carpenter's sixth proposition, that vaccine protection depends upon the number of marks left upon the arm. But this view disagrees with Dr. Cameron's admission, that the increase of the disease had been most striking in the *best* vaccinated class of cases.

A number of hospital reports, quoted by Mr. Taylor, show that the percentage of cases and of deaths bears no ratio to the number of marks traceable on the arm. Thus the "Report of the Metropolitan District Asylums" gives a table of deaths of children under five years old from small-pox. Of those which had been vaccinated at all the percentages were—

One mark	22 per cent.
Two marks	28 ,,
Three marks	18 ,,
Four marks	0 ,,
Five marks	16 ,,

A similar table for persons between thirty and forty years of age runs thus:—

One mark	16 per cent.
Two marks	20 ,,
Three marks	21 ,,
Four marks	23 ,,
Five marks	8 ,,

We think no causal relation whatever can be deduced from a discussion of these tables.

But if the failure of "the vaccination system, as at present carried out," is due to a deterioration in the quality of the vaccine lymph employed, the question occurs how and whence is a better supply to be obtained? Here there is, unfortunately, a want of unanimity among high professional authorities. Dr. Carpenter recommends "calf-vaccination." Jenner maintained that so-called spontaneous cow-pox does not protect against smallpox. Prof. Symonds, of the Royal Veterinary College, as quoted by Mr. Taylor, does not believe that any form of variola belongs to horned cattle. We may first inoculate a heifer with smallpox, and apply the matter thus obtained to human subjects. But the result, according to some physicians, is merely a "mitigated smallpox." We find ourselves thus led to Dr. Carpenter's eighth proposition, in which he makes light of the practical danger of the communication of other diseases, especially syphilis and scrofula, by the transfer of vaccine matter from one person to another. This is in reality the turning-point of the whole dispute. If such danger exists, and if it cannot by any precaution be removed, the protest against compulsory vaccination is—to use the mildest expression—perfectly justifiable. It is surely far better that a child should die of smallpox than live as a scrofulous or a syphilitic subject, and in all probability communicate these horrible diseases to others. Now the medical opinions and the professional evidence quoted in Mr. Taylor's pamphlet are by no means reassuring. Sir Thomas Watson, referring to this danger, speaks of vaccination as involving a "ghastly risk." M. Ricard, though a supporter of vaccination, admits that it must be abandoned *if but one case of the inoculation of syphilis could be traced.* Dr. Warlomont says—"In any country where it is obligatory upon parents to have their children vaccinated, the State is under a moral obligation to furnish families with a vaccine which is beyond the risk of all suspicion. Mr. Brudenell Carter ("Medical Examiner," May 24th, 1877) thinks that "syphilitic contamination of vaccine lymph is by no means an unusual occurrence," and further, that "a very large proportion of the cases of apparently inherited syphilis are in reality vaccinal." Mr. Hutchinson pronounces the danger of transmitting syphilis "real and very important." Dr. Ballard, a medical officer of the Local Government Board, writes in a prize essay that "there are numerous cases on record to prove that the vaccine virus and the syphilitic virus may be introduced at the same spot, by the same puncture of the

vaccinating lancet." Dr. Cameron, in a letter to the "Times," states that in 1867 the chief of the French National Vaccination Service published an account of upwards of 160 cases of syphilitic infection, through vaccination, which had been brought under his notice in little over a year."

A Parliamentary Return shows that whilst general infant mortality has been decreasing, the deaths from those nine diseases which are capable of being communicated by vaccination have largely increased. Of 2000 deaths registered as due to syphilis in 1874, 1484 (or nearly 75 per cent) were infants under one year of age.

Dr. Carpenter seeks to set these grim facts aside in a manner which we must decline to characterise. He contends that as we do not refuse to drink water because it may contain lead, so we should not refuse to be vaccinated because the lymph may contain the syphilitic virus! He forgets that it is easy to detect lead in water, whilst to discover the presence of syphilitic poison in vaccine is an unsolved problem. Further, not one nor a hundred glasses of plumbiferous water would establish hopeless lead-poisoning, whilst one minute particle of syphilitic virus introduced into the system may defy elimination. Lastly, even a fatal case of lead-poisoning is of much less consequence to society than a case of the transmission of syphilis. The former evil cannot be communicated by the victim to others. Nor must it be forgotten how greatly the danger is multiplied by the modern plan of repeated vaccination.

If we reject human lymph—a precaution especially requisite in a country like Britain, where syphilis has been in a manner cherished as a moral agent—we encounter, it would seem, other dangers on the nature and probability of which further information is needed. Mr. Taylor contends that vaccination direct from the calf may convey bovine tuberculosis, and that matter obtained directly or indirectly from the horse may implant glanders—an evil worse, if possible, than syphilis itself.

We are now in a position to summarise what has been said. Both Mr. Taylor and Dr. Cameron agree that vaccination, as now performed, falls very far short of giving complete protection, and that such protection is gradually decreasing. But Mr. Taylor goes much further: he pronounces vaccination a "rite," a "superstition," whose "failure begins from its earliest institution." This extreme view he does not prove, and, making all allowance for a temporary abatement of the virulence of smallpox at the

beginning of the century, it is difficult to see how vaccination could have acquired its hold upon the faculty without a solid basis of results. Even in the present day facts favourable to the "rite" are not wanting.

But on the ground of the possible introduction of scrofula, syphilis, &c., we are compelled to admit that Mr. P. A. Taylor has made out a case, if not for the abolition of "compulsion," at least for a broader and more thorough inquiry than the question has yet received. In a matter of so much importance we cannot afford to remain in the dark.

IV. ASTRONOMICAL NOTES.

By O. Reichenbach.*

From a Letter to D. P. Todd, Esq.

THE coincidence in longitude between your planet and Prof. Forbes's inner planet appears "remarkable." Yet it must be as accidental as the approximate coincidence between the longitudes of Prof. Forbes's two planets and my A and B, because the differences in distance are too great.

The coincidence between your planet and my A, on the contrary, presents a considerable probability for the existence of both A and B, and for the more or less correctness of their present positions, because $a=46$ or 52 as you take it, and $a=42$ as I made it twenty-four years ago, differ comparatively little, and because our findings rest on similar bases, the influences of planet upon planet, and are not remotely as speculative as the assumptions of Professor Forbes.

You say there was a conjunction between Uranus and A, as I call it, in 1780 to 1793, and will be another before the close of the century. If A is at present in 196°, there was such conjunction in 1778, and will be another in 1904; if it be in 201°, it was in 1780. You get longitude 170° or less, I 196° or more: that you finally take $a=52$, and I have $a=42$, explains all differences in time of conjunction and present longitude; we completely agree when we both

take 42. I said in 1875 B is in 305° with error in more, say 308°; its distance is 76, not 300 as Prof. Forbes has it: there would have been a conjunction with Uranus between 1824 and 1829, it being at the time in opposition. When you examine the "residuals" in your article you will find this conjunction well indicated, principally when you adopt the names I got for A and B in my development theory in 1857.

In the papers Le Verrier read before the French Academy in 1846 ("Connaissance du Temps," 1849, p. 240) he said—"Erreur presque toujours la même entre 1824 et 27." "Valeur la plus précise 36·154 ne peut varier qu'entre les limites 35·04 et 37·90." "207 a 233 années envirez."

In 1857 I got for Neptune mass 1-14446, pretty near that made out by the two I have; and I believe I can show why the figures since obtained by Prof. Newcomb—the one by Uranus, the other by a satellite—are more at fault.

From a Letter to Prof. GEORGE FORBES.

Is the grouping of comets always determined by one planet only? Are the distances of the two planets inferred from the mean distances of the groupings the true ones? Does not your cometary method give too free play to the hypothesis because the number of periods elapsed since the catastrophe is selective? On the other hand, is it not too great a restriction to require such close coincidence in longitude, and to require that the determining planet should always have been the one having its orbit next to the present aphelion of the comet? Might it not have been the one which was next to that aphelion position at the time of the catastrophe, when the planet with the nearest orbit was far away?

Some comets may have been born within our system; others may enter and leave it; still others may continue in it, and may get condensed, or divided and absorbed.

The incorporation of the comet is the result of the reduction by additional planetary attraction of an original surplus of helio-centrifugality, which carried the comet from another system into ours, and would carry it out again without this disturbance.

We may consider some aphelia of the Jupiter group as originally produced by the inner planets and determined by Jupiter, but all crowded round the greatest of planets.

Between Saturn and Uranus there is no whole group of cometary aphelia, but probably a cluster of smallest planetoids

between Saturn and Uranus. The six comets of the "Neptune group" have their aphelia beyond that planet inside 35 "Earth's radii," inside the first half of the distance from Neptune towards A, at distance 42.

Comets 1857 IV. to 1855 II. all have their aphelia near and somewhat beyond 76 "Earth's radii," the distance I got for B, to which I assigned a considerable eccentricity.

The position of B comes next to that distance which you give to your inner planet. Most of the aphelia of this group exceed $a=76$ for B in like proportion, as the aphelia of the first group exceed $a=30$ for Neptune.

The group from 285 to 420 "Earth's radii" is the reason that you place a second planet at 300 such radii: 242 "Earth's radii" equal 524,282, or solar radii fall some distance inside 300 such radii, and play, in my development theory of the solar system, a prominent part.

The last comet on the list, at $a=4275$, falls little inside I C, the radius of 9,248,007 of the *motion volume* of the solar system.

To show that your method may also work in favour of the distances and positions I obtained for A and B, I take the Neptunian group, because that planet is known, and because A at 42 corresponds to your planet placed by hypothesis at 100, whose position you consider best established.

The group lays entirely within the first half of the proportionally next great distance between Neptune and A, which I took to be at present at 201° with error in less. You mention a star observed in 1860: supposing it was A, it would be now about 196°. I take this instead of 201°.

Of Comet I., 1852, iv., you say "It does not seem to fit in" with Neptune. Its aphelion falls in 215°. At the time of the passage in 1607, A was at 202°.

Comet III., 1815, has its aphelion in 321°; it passed it in 1500, 1570, 1640, and 1710. In 1500 Neptune was in 296°, and in 1710 A was in 335°.

Comet II., 1812, is said to have passed its aphelion in 1635 in 256°; Neptune was then in 242°, and A in 245°.

Comet IV., 1846, iv., and V., 1847, v., of which the aphelia in 260° about coincided in 1810 with the longitude of Neptune, had the nearest coincidence with A in 1666 and 1660 respectively, at 20° and 12° difference.

It is characteristic for cometary changes that I. 1852, iv., of which you say "it does not fit in" with Neptune, but fits so well with A, has its aphelion nearest to Neptune within the group; and that the two last comets, which in their last revolution do not fit with A, but so well with

Neptune, are the nearest to A, about half-way between the two planets.

To fix the distance and position of your nearer planet you have a group of seven comets, of which five had their last aphelion within thirty years from each other.

B is at $a=76$. It is inside and next to the planet you assume; it is, I said, in 308° with error in more. You have accommodated your distances to the requirements of your hypothesis; I have obtained my distances on other grounds, and see when and where the comets meet my planets as nearly as possible, in not too remote periods.

For I., 1840, iv., you carry the catastrophe back to 968. There was an aphelion passage in 1668 in 220°: this would correspond to B at present not in 308°, but in 335°; or at present in 308°, and in 1668 in 193°.

Of III., 1846, vii., and V., 1793, ii., you say they do not "fairly fit in." The first of these comets has its aphelion in 340°; you carry the catastrophe back to 1248, with the planet in 320°. Taking B now in 308°, it would have been in 1248 at 325°. The second comet passed its aphelion in 241° in 1160; B was then in 276°, if it be now in 308°.

II., 1843, i. does not fit in with B, and you refer it to a planet at $a=100$, which was in 98° when the comet was in its aphelion in 100°, in 1655. In the period of 1250 A was in 100°.

IV., 1861, i., aphelion longitude 37° coincidence with your planet in 409; A was in the period of 1239 in 85°.

VI., 1861, ii., aphelion in 96°, coincided with your planet in 1651. In the period of 1232 A was in 76°.

VII., 1855, ii., aphelion in 79°, met your planet in 1609, in 82°; agrees neither with B nor A, but Schulze deduced an ellipsis with an aphelion distance of 57 instead of Donati's 124·2.

V. THE PSYCHIC CALENDAR OF CREATION.*

UNDER this remarkable title Prof. H. A. Reid, Secretary of the State Academy of Sciences at Des Moines, Iowa, gives an original and interesting classification of the organic world, according to the successive develop-

* Kansas City Review of Science, September, 1881, p. 264.

ment of the organs of sense. The order of the "successional types of animal life" appears to him approximately coincident with the geological order, and both lead up to "the perfect harmonisation of the law and philosophy of Evolution with the grand distinguishing doctrine of the Christian Bible, that there is a 'natural (or carnal) man' and there is a 'spiritual man.'" It is doubtless not unknown to many of our readers that the most eminent American biologists find in the doctrine of Evolution, and in its applications, nothing necessarily incompatible with the general idea of a personal God, and in particular with the Christian revelation. In this respect they differ strikingly from a large section of the German, and even of the English, naturalists, who lean towards the materialistic view of the universe. A further distinction which must not be overlooked is, that in the United States Darwinism pure and simple, as it is commonly but doubtfully called,—*i.e.*, the ascription of the origin of organic species to the fortuitous accumulation of minute variations,—is less widely accepted than in Europe. Very few believers in the transformation of species fail to see that before Natural Selection could be brought into play Variation must have existed.

The former of these characteristic features of American Evolutionism scarcely falls within our usual sphere of discussion, and into the latter we are not about to enter at present. But both of them will serve to explain the position taken by Prof. Reid. This author gives, in illustration of his lectures on "Evolution, Science, and the Bible," a tabular view in which all living beings are divided into seven groups. First and lowest come the *Insentia*, primordial fucoids and vegetation generally, having organic forms, but no motative sensibility.

The next step brings us to the *Unisentia*, *i.e.*, sensitive plants, rhizopods, pseudopods, protoplasm; the eozoon. These beings are said to possess the sense of touch only.

As the third group figure the *Disentia*, to wit protozoa, polyparia, radiates, mollusks, having—according to the author—taste and touch.

Fourthly come the *Trisentia*, *i.e.*, sauroid fishes, crustaceans, trilobites, and worms, having—we are told—sight, taste, and touch.

In the fifth class, or *Quadrisentia*, rank humoid reptiles and palæozoic sea-saurians, endowed with smell, sight, taste, and touch.

Next follow the *Quinsentia*, including brute man, mam-

mals, birds, and aerous reptiles, possessing hearing, smell, sight, taste, and touch.

Lastly come the *Sexasentia*, the "spiritual man" of the Bible, having, in addition to all the former senses, a spiritual sense.

The first remark which will occur to the practical naturalist is, that Prof. Reid has found no place for the most numerous of all the great primary groups of the animal kingdom,—*i.e.*, the insects. That they possess *at least* five senses is indisputable, and there is a possibility that some of them may even have more. Then we note that the Crustaceans are thrust down into the Trisentient group, and declared void of hearing and smell—a very hazardous assumption!

The Mollusca fare still worse, and are pronounced to have taste and touch only. Yet the Cephalopoda, the highest molluscous type, have very distinct eyes, and also organs of hearing.

As unisentient we find "protoplasm" figuring as a distinct animal, or vegetable species. As to the eozoon, its organic character is still *sub judice*, and any expression as to the number of its senses is therefore somewhat indiscreet.

Lastly, it may be question whether vegetation generally should rank lower than the simplest forms of animal life.

We do not dispute, as Paley suggests, that "there may be more and other senses than those which we have." We have speculated on the possibility of such senses, and are even now searching for phenomena which may demonstrate their existence. We would also agree with Prof. Allman that "in the far-off future there may be yet evolved other and higher faculties."

The author advances the opinion that Man is an "embryonic and transitional type," and not a "closed or terminal type like the Quadrumana and other lower forms." It may be noted that there is at least one instance of an animal reproducing itself not in its highest stage, but in an antecedent and inferior condition. We will likewise not forget the fact that the embryonic stages of certain animals remind us of the mature structure of lower types. But let us suppose Prof. Reid to say, if formally challenged to produce scientific evidence, that man is a larval, transitional form. We fear the analogies above mentioned, and others which might be cited, will scarcely be accepted as decisive.

VI. A BRACE OF PARADOXES.

MANY persons at the present day are not only willing, but anxious, to submit their most confirmed practices and their most deeply-rooted convictions to a re-examination. Of this tendency no one can justly complain provided the scrutiny is carefully and seriously undertaken. But it sometimes happens that old customs and established opinions are challenged on the strength of some desultory one-sided observation, or of some hasty and imperfect induction. In such cases the logic of Science bids us pause and most carefully weigh the evidence submitted to us before adopting the novel views. The history of discovery tells us of many undoubted truths which, when first propounded, were met with opposition, ridicule, and even downright persecution. But it can tell us little of the sham facts, of the baseless hypotheses, of the mad philosophies which are continually springing up like toadstools in an autumnal wood. The authors of such phantasies are always ready to pull down and re-erect at brief notice the whole fabric of modern Science; and if learned societies and editors of journals turn a deaf ear to their lucubrations, they pose in the character of a neglected Lamarck or an imprisoned Galileo, and warn us that posterity will do them justice. Britain has been particularly rich in such men. But in this respect, as well as in certain others, we may yet find ourselves outstripped by our kinsmen across the Atlantic.

We propose, in illustration of this subject, to take up two new biological notions, lately put forward in America, the one of a practical and the other of a speculative character, and examine the evidence upon which they are founded. We are far from asserting that the theories in question have been promulgated from the mere love of notoriety, or from dissatisfaction with existing customs and opinions simply as existing. But we think that there has been a great lack of thorough examination and a degree of rashness not entirely free from blame.

The first of these novel doctrines relates to the mastication of food. We have all been told by medical authorities, from Abernethy downwards, that to bolt our food rapidly in large lumps interferes with digestion, throws upon the stomach an additional burden, and thus injures health. This advice, be it remembered, was not founded upon

chemico-physiological investigations, but upon experience and observation. Theories concerning the action of the saliva, and anent comminution as exposing a larger surface to the action of the digestive fluids, were added subsequently in explanation. Further, if food is to be bolted, not chewed, what is the particular necessity for teeth and for saliva at all?

It must also be considered that whether we owe our teeth to an act of special creation or to the development worked out under the influence of natural selection, the case will be the same. Yet, in face of these considerations, deliberate feeding and thorough mastication are now condemned as not only needless, but positively hurtful, interfering with the work done in the stomach instead of promoting it. Dr. W. Browning, in order to decide the question, made a series of experiment upon dogs, and says:—"If the meat, before being fed to the dog, was reduced to hash or cut into fine pieces, the digestion was at best but imperfect, a considerable portion of the undigested or imperfectly digested meat being found in the excreta. If, under the same conditions, meat was fed to the dog in large pieces, it was bolted at a gulp, with the result that little, if any, passed through undigested: compared with the result from the chopped meat, it could be called a perfect digestion for the coarse form as compared with a decidedly imperfect digestion for the fine form. So far as simple experiment goes, this must be pretty conclusive for the dog."

From these facts he reasons, in our opinion illogically, that the same ought to be true of human digestion. Illogically, we say, because the circumstances of the cases differ in every important particular. Meat, the main diet of the dog, as of all Carnivora, is admittedly easier of digestion than other articles of food. Hence we see that the teeth of the Carnivora are adapted not for mincing and grinding, but simply for cutting and tearing flesh into morsels not too large to be swallowed. The more exclusively and truly carnivorous any animal, the more its teeth display this type, whilst in proportion as any species inclines to a mixed diet the more its teeth are fitted for grinding. If we, then, mince the food of a carnivorous animal we place it under abnormal circumstances, and must not wonder if its functions are disturbed. With man the case is very different; his food is more or less composed of vegetable matter; his teeth are adapted for grinding to a pulp, and his intestines are relatively larger than those of the dog. If we may logically argue from the dog to the man, why not also to the

horse, and even the cow? Shall we tell the former that careful grinding and the latter that rumination are respectively mistakes?

Prof. Ludwig, it seems, has made some experiments upon himself to test this theory, eating coarsely-cut meat at one time, and at another meat finely divided. He reports that he "has not been able to detect any evil consequences from swallowing mouthfuls as large as he could get down." We dare say not; he has, in the first place, experimented with meat, the most digestible of foods, and the one which does not require the action of the saliva. Let him try with bread, potato, lentils, carrots, &c., bolting them in the lump. It is not stated, further, how long his experiments have been continued. The results of an unwholesome way of living often do not make themselves felt for years.

We have never purposely made any experiments with reference to this question, but we have often noticed that after a hurried meal oppression of the stomach and other symptoms of indigestion made themselves felt. Often have we heard from acquaintances similar complaints of the action of food swallowed in haste, and without due mastication.

Dr. J. F. Hibberd, quoted in the "Boston Journal of Chemistry," says that healthy people generally eat fast, while those that are unhealthy eat slow. Were this rule general, which we are far from admitting, it would bear a very different interpretation from that which he seems to suggest. Healthy people, especially when young, often take liberties with their constitutions which the feeble and sickly cannot venture upon, whilst the unhealthy have often learnt by sad experience to avoid unwholesome practices. Suppose we were to say "People with sound strong teeth generally bite string and thread, straighten pins, and crack nuts with the mouth, whilst those with decayed and loose teeth do nothing of the sort." Would it be logical to argue that the nut-cracking and thread-biting were the causes of the good teeth of the former? Yet this is exactly Dr. Hibberd's style of reasoning. Speaking of teeth we are reminded that, if the new theory is well founded, their day—be they natural or artificial—is over. Wherefore let the dentists look well to the matter.

The second paradox which we shall notice was brought forward at the Cincinnati meeting of the American Association for the Advancement of Science, by Dr. C. S. Minot, of Harvard University. He read a paper entitled, "Is Man the Highest Animal?" and answered his question in the nega-

tive! Now many of our readers are doubtless aware that scientific notions concerning the relative rank of different forms in the animal kingdom are at present in a very unsettled condition. The old view that animals rise successively higher and higher as they approach nearer and nearer to man has been challenged in several quarters. It is inconvenient for those who wish to establish a want of continuity between man and the rest of the animal kingdom. Thus an eminent biologist who maintains this view, pronounces the cats "the very flower and culmination of the Mammalian animal tree," and declares that man, "considered merely in his capacity as an animal, has a very definite place in such a scheme, but it is by no means certain that his place is at its summit;" and again, "the close bodily resemblance of the apes to man gives them no just claim to a rank above that of the Carnivora, since such a claim only reposes on their bodily resemblance to ourselves." Thus Professor Minot is not entirely taking a new departure. He maintains that the measure of biological rank is the degree of specialization exhibited by all the organs, taken collectively. The measure of specialization he finds in embryology, "which shows in earlier stages simplicity and uniformity of structure, which in later stages is replaced by complexity." We quote here the author's own words because we shall have shortly to turn them against himself. He declares that "specialization may be exaggerated in one or several organs without the animal attaining a high rank as a whole, and this he pronounces to be the case in man. In the human body he admits certain high differentiations. to wit, "in the brain, in the changes induced by, or accompanying, the upright position, and in the opposability of the thumbs to the fingers." In these points he admits man to be "more highly specialized than any other animal."

On the other hand he finds in man, "instances of a still more striking inferiority." His senses are less acute than those of many animals; "he has neither the keen vision of the falcon nor the delicate scent of the dog." On this we may remark that though the sight of birds of prey is telescopic, yet that in them nice discrimination of colour is not proven. Man cannot have so acute a scent as the dog, because the development of the brain leaves little room for the olfactory lobe. Prof. Minot goes on to assert that man's teeth are of a "low mammalian type," and that "similar inferiority is shown by his limbs, since they are little modified from the primitive (embryonic) form, preserving the full number of five digits. In respect of these members

man stands, therefore, very low—lower than the cow and the pig."

We must here point out that a departure from the original embryonic type may be in one of two directions; it may be progressive, passing, as the author has above laid down, to greater complexity, or it may be retrogressive or degenerative, tending towards less complexity. All the modifications of the limbs which reduce the number of digits are, therefore, cases of degeneration, and the author, in pronouncing man's five fingers or toes a mark of inferiority to the structure of the cow or the pig, contradicts what he has advanced above. Again, he has admitted the "opposability of the thumbs" as a mark of high specialization. In afterwards ascribing superiority to an extremity where the digits are undifferentiated, and reduced to two or to one in number, he is guilty of a second contradiction.

Again, in the ruminants, &c., the four extremities are scarcely at all differentiated in structure or function. They are mere supports, adapted for locomotion. As we pass through the Carnivora and the apes to man, we find the two pairs of extremities more and more differentiated—a familiar instance of man's superiority which the author has ignored.

Again, we read that man "plants the whole sole of his foot upon the ground, yet none but the lower Mammalia, together with man and his immediate congeners (a formidable exception!) are plantigrade." Now if man is to stand on two extremities only, which Prof. Minot has just mentioned as a mark of high specialisation, he must be plantigrade! Here, therefore, is a further case of self-contradiction.

The author proceeds, "So, too, with his stomach, which is so simple as compared with that of a ruminant, and, indeed, is of about the same grade as that of the Carnivora." Surely the stomach of the ruminants is a mere adaptive feature suitable for animals which require to consume a huge bulk of matter poor in nourishment. But it is important to see how Mr. Minot plays fast and loose. In case of the foot and hand he proclaims complexity a mark of inferiority. In the stomach he holds it to be a sign of superiority! He is consistent only in inconsistency and self-contradiction.

Further: "It makes a still more forcible impression to learn that the human face which we admire when withdrawn under a high intellectual forehead, is perhaps the most remarkable of all the indices that point to man's inferiority. In the mammalian embryo the face is formed under the

forebrain; in our faces the fœtal disposition is retained with changes which, when greatest, are inconsiderable. In quadrupeds the facial regions acquire a prominent development, leading to the specialization of the jaws and surrounding parts, which brings the face to a condition much higher than that of the fœtus. Hence the projecting snout is a higher structure than the retreating human face."

If this doctrine holds good it must avail within the human species, and hence prognathous races will be entitled to rank higher than normal man. But how is the prognathous type acquired? Banish a tribe to some unhealthy climate and feed them on insufficient and unwholesome food, and they will become prognathous, and at the same time stunted. Perhaps Prof. Minot would pronounce them higher than their ancestors.

The author might have done well to remember that snoutiness—if we may use the term—reaches its height in certain groups which must certainly rank among the lowest mammals, *e. g.*, the Insectivora, the Edentata, the Platypus, Echidna, &c. Yet on his showing this feature ought to be a mark of superiority.

The author's great error lies in the tacit assumption that every departure from the original embryonic type is progressive, whilst both in the individual and in the animal series it is very frequently a case of degeneration.

What animal he considers entitled to the highest rank he prudently declines to state. He adds further, "It is also doubtful whether mammals would be regarded as the highest class of the animal kingdom, were they not our nearest relatives." Here, again, he would have done well to say to what class he would assign that honour.

We refer with regret to the strong language which Prof. Minot has thought it becoming to apply to those who differ from him. The ordinary view he pronounces "a silly prejudice of arrogant ignorance." This comes with scant grace from a man who cannot reason without contradicting himself at every turn, and who in addition has not proved his superiority to the bulk of mankind by any discovery of capital moment. The future of the New Natural History will be gravely compromised by such speculations.

VII. TRICHINÆ AND THEIR DISTRIBUTION.*

FLESH-WORMS are now fully recognised as one of the most decided scourges of the human race, and a war of extermination has to be waged against them. It must not be supposed that these *Trichinæ* are a newly-created pest, or that their habits have even undergone any decided modification within historical ages. In the days when the microscope was not, the presence of these parasites in pork and other flesh was little likely to be noticed, and when medical science was in its infancy the symptoms of trichinosis might very well be confounded with those of typhoid, of malarial fever, or of acute rheumatism. Dr. Leidy thinks that, even as recently as the American civil war, numbers of deaths in reality due to the use of semi-cooked pork were ascribed to fevers.

Passing over, as beside our present purpose, the life-history of the parasite, its detection in meat, and the means recommended for its destruction if so present, we turn to the agencies for its distribution from one individual or one species to another. *Trichinæ* may be present, it appears, in swine, dogs, cats, rats, and incidentally in oxen, rabbits, and guinea-pigs. According to Mr. Phin birds and sheep do not offer an acceptable harbour to these intruders, since, even if fed upon trichinised matter, the parasites have not been detected in their flesh. We have heard, however, that certain French soldiers became fatally trichinised from partaking of the flesh of a goose. As the goose does not feed upon carrion, or offal of any kind, this case, if authentic, is the more serious. Trichinised river fishes are also said to have been detected near Antwerp.

Mr. Phin enumerates four methods in which *Trichinæ* may be conveyed from one animal to another. The general process is that the trichinised animal is eaten, wholly or in part, by some carnivorous or omnivorous creature. In this manner man, swine, rats, cats, and dogs most frequently become infected. Man is exposed to this danger only when he adopts the custom of eating pork, &c., raw, or "rare," which is the fashionable euphemism for half-raw. Among

* Trichinæ, How to Detect them and How to Avoid them. By JOHN PHIN Rochester (U. S.): Bausch and Lomb. Co.

swine the infection has become greatly increased since the abominable practice of feeding them upon butchers' offal became general; and it is remarked that the very parts given to pigs—such as the diaphragm, and the junctions of muscles and tendons—are exceedingly liable to contain *Trichinæ*. But we must beware of supposing that swine fed upon corn, fruits, mast, &c., must necessarily escape the pest. A wild boar killed in November last, near Khiam, in Syria, according to the "Lancet," communicated trichinosis to two hundred and sixty-two persons, several of whom died. It appears that the wild boar, when grubbing in the earth for roots, frequently snaps up and devours rats, field-mice, &c. The habits of tame and half-wild swine, when turned out into the forests, are precisely similar. Even when in the sty, pigs have been known to capture and devour the rats which came to steal their food. It often happens, also, that rats which have been worried by dogs—who rarely, if ever, eat them—are left in the way of pigs, and get eaten. Dogs are much given to prey upon slaughter-house offal, yet are seldom found trichinised. Mr. Phin has never met with an authenticated instance. Cats, on the other hand, according to Dr. Seiler, of Philadelphia, are very frequently infected. Mr. Phin considers that the cause of this lies in the fact that cats eat rats, while dogs never eat them. We can scarcely accept this contention. We have known cats who were adepts at ratting, but who never devoured their prey. Others, if very hungry, will eat the liver, and sometimes the back of the neck, which it must be admitted is a very likely habitat for *Trichinæ*. Cats, however, according to Dr. T. Spencer Cobbold, F.R.S., are exceedingly subject to parasites.* Rats which prey upon slaughter-house offal are often trichinised. Pikes may conceivably become trichinised by devouring rats in the rivers.

The question may here be raised, What animal is the original *nidus* of the *Trichina?* At present we see the pig receiving them from the rat, and the rat again becoming infected by devouring pork. But we are unable to lay our finger on the beginning of the series.

A second source of infection must be sought in the excrements of animals which have recently become infested with *Trichinæ*. Mr. Phin observes that the pig, the rat, and the dog "are all ravenously fond of excrementitious matter, eating it greedily when they can get at it." So, we may

* Internal Parasites of our Domesticated Animals, p. 124.

add, are domestic poultry, as well as slugs and snails. When any animal has recently been feeding on trichinised matter its excrements will contain young *Trichinæ*, and even mature females who have not yet brought forth their progeny. In this manner, among dung-eating species, one individual may affect the herd. Even animals not intentionally and consciously devourers of excrement may contract trichinosis if the fæces of rats or mice are mingled with their food or drink. Mr. Phin suggests that the hippopotamus which died some time ago at the Zoological Gardens may have become infested with *Trichinæ*.

A third manner in which these intruders may be spread from animal to animal, if less frequent in its occurrence, is more insidious, and may reach creatures which eschew impure food. Let us take the very possible case that a trichinised dog, cat, or rat, is drowned in a pond or river, or that its carcase is thrown into the water in that nasty spirit which dooms our streams to be the common repositories for filth. The body putrefies, and the *Trichinæ* present in its intestines, or encysted in its flesh, are set at liberty. Will they not be destroyed by the process of putrefaction? No; unfortunately, like most nuisances, vegetable or animal (human included), they are very tenacious of life. They have been experimentally kept for months in putrefying animal matter without apparent injury. If, therefore, a cow or a horse happens to drink the water near the place, it may receive into its stomach a number of *Trichinæ*. Now to prevent dogs, cats, and rats from being occasionally drowned in our rivers is a consummation devoutly to be wished for, but we fear beyond the power of the most stringent legislation.

So far as we have gone the vegetarians may tell us that if we suffer from trichinosis we are justly punished for eating "impure" food. But their turn is now to come. Even vegetables may be contaminated. If plants are manured with the offal or the excrements of trichinised animals, they may communicate the infection. For man the chief danger is in case of lettuce, celery, radishes, and herbs eaten raw.

Bearing in mind the facts that *Trichinæ* are not in any moderate lapse of time destroyed by contact with putrefying animal matter, fæces, &c., and that they can bear prolonged immersion in water without injury, we may well assume that they will occasionally be present in sewage. Pigsties, slaughter-houses, &c., will occasionally be washed down into the sewers; they may happen to contain *Trichinæ*. Let us now suppose that such sewage is used for irrigation, and is

turned over pastures or market-gardens, as at Gennevilliers. It is perfectly possible that man and herbivorous animals may in this manner receive the infection.

It is interesting to notice the very illogical manner in which irrigationists have sought to disprove the danger of the transfer of *Trichinæ*, and of Entozoa in general, through the agency of their favourite scheme. They have, forsooth, fattened an ox upon sewage-grown vegetables, and have then submitted the carcase for microscopic examination to competent authorities; and no signs of the presence of internal parasites having been discovered, they have ventured to proclaim the danger entirely imaginary! But no one has asserted that *every* sewage-irrigation field must *at all times* receive Entozoa or their germs, and that *every* animal fed upon the produce of such fields must be infected. There is merely the great probability that such may be occasionally the case, and in virtue of the very serious character of the consequences our only safe course is to treat all sewage-grown vegetables, and all animals fed upon such vegetables, as suspicious. A man adopting the irrigationist logic might say "I have eaten a slice of raw ham, or a raw sausage, or an underdone pork-chop, and have suffered no injury: therefore *Trichinæ*, if they exist at all, are harmless!" So long as we know that Entozoa can, and actually do, exist in sewage, so long we are warranted in protesting against its direct application to crops which are to be eaten in an uncooked state, either by man or beast.

It must of course be admitted that the number of *Trichinæ* which may be introduced into an ox by a draught of infected water or a feed upon contaminated grass, clover, or cabbages, or into a man through the medium of a salad or a stick of celery, is very small. But unhappily the pest is rapidly prolific. Each female brings forth about 10,000 young, and if we assume that half the adult *Trichinæ* are males, and that a considerable proportion are expelled from the body of their host, along with the excrements, before they have time to bore their way through his stomach and bowels and enter his muscles, we may assume, with Dr. Cobbold, an average progeny of 3000 for each *Trichina* swallowed. Phin, for argument's sake, takes only half this number, and calculates accordingly that if a rat, feeding on butchers' offal, swallows two dozen *Trichinæ*, it will shortly have 36,000 of their progeny encysted in its muscles. Suppose the rat to be in due course devoured by a porker, the latter will speedily contain a population of some fifty millions, and its flesh, if eaten by human beings in doses of (say) half a

pound, without thorough cooking, may bring on fatal attacks of trichinosis. Thus the rat, so to speak, condenses and concentrates the mischief.

There are certain modern changes in our habits which are very favourable to the spread of Entozoa in general, and of *Trichinæ* in particular. Imperfect cookery has, indeed, always ranked among our national sins, and we still cling to the semi-barbarous habit of submitting our animal food to the action of fire in such large masses that the heat cannot penetrate to the centre of such sparingly-conductive matter. But in former days we were always, as far as the present writer has been able to learn, much more careful about the thorough cooking of pork than of beef or mutton. Latterly, however, we seem to have caught the German superstition that smoking meat may be accepted as a substitute for cooking. We do not believe that Englishmen have yet been persuaded to eat raw ham, "hack-fleisch" (*i.e.*, raw meat), minced or "speck-salat,"—to wit, salads in which bits of raw bacon do duty instead of oil. But we fear that smoked (and uncooked) sausages are gradually finding their way into consumption. Parenthetically we may point it out as something exceptional that we, who as a nation are very conservative in our diet, should have been so quickly taught to relish the flavour of burnt wood.

Some people have come to believe that the operations of salting and smoking may kill the *Trichinæ*. As far as the surface of a ham is concerned this may be the case. But the quantity of salt and smoke necessary to penetrate the centre of a thick piece of meat, and destroy the *Trichinæ* right through, would be more than sufficient to make it indigestible, and indeed quite uneatable.

The action of fire itself, as commonly applied, is not sufficiently prolonged to destroy life to the very centre. As Mr. Phin remarks, "the cooking must be *thorough;* no mere surface-scorching, on the one hand, or brief dipping in boiling water, on the other." A temperature of at least 160° F.—we should say rather 212° F.—must be reached in every part of the meat. It is, indeed, maintained that so long as any part when cut looks red, or emits a red juice, so long there is no safety,—a result manifestly unattainable if the meat is cooked in large masses. It is exceedingly unfortunate that in these days such a great proportion of the population are compelled to dine at eating-houses, or to buy their food at "ham and beef shops," in both which classes of establishments semi-rawness is the rule.

On the prevention of *Trichinæ* Mr. Phin has various

suggestions to make, which if followed out would certainly lessen the evil. He would prohibit feeding pigs on raw offal of any description. "All animal matter fed to pigs must be chopped into pieces not larger than a cubic inch, and thoroughly cooked." This process, he considers, would not only destroy the parasites, but so improve the food that the extra expense would be more than made up.

This recommendation is right to a certain extent, but we should propose a more decisive step, *viz.*, the total prohibition of animal offal, blood, &c., being used for feeding swine, or any other beast consumed as human food.

Secondly, Mr. Phin would forbid the use of slaughter-house manure or offal on pastures or "land on which are grown vegetables to be eaten in a raw state by man or beast." This prohibition must, by a parity of reasoning, be applied to sewage. The use of blood, offal, &c., might be rendered perfectly safe by a process of torrefaction at about 250°. At this temperature no valuable principle is driven off, and, according to the experience of certain French authorities, animal matter thus treated is more available as plant-food, whilst all Entozoa would certainly be destroyed. The slaughter-house refuse at present given to swine would find a useful application as manure after being submitted to this process.

Sewage-irrigation would of course be very much restricted in its applications, but manures obtained by precipitation would be freed from *Trichinæ* and other Entozoa by the temperature employed in drying the deposit.

Mr. Phin's pamphlet may be regarded as exceedingly timely in giving, in a condensed and intelligible form, correct information on a newly-recognised danger.

ANALYSES OF BOOKS.

The Formation of Vegetable Mould through the Action of Earth-Worms, with Observations on their Habits. By CHARLES DARWIN, LL.D., F.R.S. London: John Murray.

MANY persons will probably consider the work before us un-Darwinian. It contains, indeed, no reference to the origin or the transformation of organic species, or to the theory with which the name of our great English naturalist will be for ever connected. But it is characteristically Darwinian as summing up the effects of a continually recurrent cause, each of whose manifestations may be feeble, whilst their total result is mighty. The ability thus to trace the working of agencies is one of the most prominent features of the author's mind. Another characteristic of the writer is the patience and perseverance with which, in this as well as in his other researches, his ideas are elaborated and tested prior to final publication. More than forty years ago Mr. Darwin read before the Geological Society a short paper on the formation of the so-called vegetable mould. He referred to the fact that ashes, burnt marl, &c., which have been spread over the surface of meadows, gradually disappear from view, and in the course of a few years may be found in a layer at the depth of some inches beneath the turf. This phenomenon, which farmers often ascribe to some "kind of alacrity in sinking" possessed by bones, marl, lime, and other materials spread upon the land, was traced by the author, at the suggestion of his friend Mr. Wedgwood, to the agency of earth-worms. The theory was opposed by M. d'Archiac, Mr. Fish, and others; but Mr. Darwin has not merely fully proved it by a series of prolonged and careful measurements, in which his sons and several of his friends have taken a part, but he has further shown that these humble creatures are geological agents of no small importance, aiding in the disintegration of rocks, in the denudation of the land, as well as in the preparation of the soil for the growth of plants.

The inquiry necessarily involved a careful examination of the habits, powers, and faculties of the earth-worm. Everyone knows that this little animal lives in tubular burrows in the earth, which it rarely leaves except before evening; that it travels in the night, especially in damp, rainy weather, leaving long tracks in the mud and sand. Everyone must also have noticed their so-called "castings,"—small heaps of earth which they bring up to the surface of lawns, meadows, &c. The worm in making its burrows pushes the soil partly aside, and eats the

rest. All animal and vegetable matter which is in a suitable condition is digested and assimilated, whilst the residue, very finely divided, is ejected in the form of little globular pellets. With this matter they line the walls of their burrows, but much of it they bring to the surface. The weight of each casting is not very great; but in order to estimate rightly what worms can do, we must, following Mr. Darwin, consider their number per acre, and form an idea of their work over a long series of years. Prof. Hensen, in a treatise on the habits of worms, calculates, from the number which he found in a measured plot of land, that there must be upwards of 53,000 worms per acre, or, taking their average weight at 15 grains each, about 356 lbs. Mr. Darwin found in a single cake of earth, of the size of two open hands, seven worm-burrows, of the diameter of goose-quills.

A gentleman, mentioned by Mr. Dancer in the "Proceedings of the Manchester Philosophical Society," having upset some barrels of sour ale on his land, was amazed at the heaps of worms which lay dead on the surface. We have often noticed the multitudes of worms which fall into small water-courses cut in grass-lands for irrigatory purposes. If we also remember to what extent they are devoured by moles, birds, hedgehogs, slugs, and other enemies, without growing scarce, we must admit that their numbers are very great. The weight of the castings found at the mouth of each burrow varies greatly. In the case of our common British worm it is found, by actual collection and weighing, to range from ¾ oz. to close upon 4 ozs. But the author is prepared to give more exact data. A lady friend undertook to collect and preserve for a whole year the total worm-casts thrown up on two separate square yards of ground, near Leith Hill, Surrey. From one of the plots—a place unfavourable for the life of worms—the year's castings, when dry, weighed 3½ lbs. The other plot gave 7½ lbs. The former consequently would show 7½ tons of dry earth per acre, raised to the surface by the activity of worms, whilst in the latter it would reach 16 tons. At the latter rate the deposit, if uniformly spread over the surface of an acre, would measure in the course of a single year 0·1429 inch, or in round numbers nearly 1½ inches. This quantity is somewhat smaller than the rate at which mould accumulates over objects left on the surface of the ground in a similar time. But Mr. Darwin points out that earth is also raised to the surface by moles, ants, dung-beetles, &c., and that a part of the worm-castings is blown away in dry weather, and washed down in time of rain. Thus we see that in the course of years every particle of the upper soil of a field or garden must have been finely comminuted and passed through the intestines of worms. It is doubtful whether their burrows contribute much to the drainage of the land, as worms generally stop up the opening with leaves, or in default with small stones. But that they admit air, and thus aid in the chemical changes going on in the sub-

soil, there is no doubt. From this point of view it is to be remarked that the contents of the intestines of worms and the castings themselves are always acid. A considerable quantity of dead leaves, &c., are also drawn into the burrows to the depth of 3 to 4 inches, and thus the organic matter in the soil is increased. As regards the acid secreted by worms, or otherwise produced in their systems, we would suggest that it is not improbably the oxalic, a solvent well adapted for effecting the chemical disintegration of many kinds of rocks. Numbers of minute stones are also swallowed, and by the mechanical action of the gizzard and their friction against each other they are in part pulverised.

It will easily be seen that any agency which raises small quantities of earth from beneath to the surface, and deposits them there, must aid in the process known to geologists as denudation. If the surface has a slope, the fine soil thus deposited will gradually be washed down by rains, and must then find its way to the nearest water-course, and ultimately to the sea. In dry weather the castings must be dispersed by the wind, and carried to leeward. Hence the difficulty of believing that "any appreciable quantity of earth can be removed from a gently inclined surface, covered with vegetation and matted with roots," is removed. It may even be suggested that worms play a part in causing large boulders and fragments of rock to travel down gentle declivities. They like the shelter of stones, and by gradually excavating and removing the soil from beneath the margins of the block they may cause it gradually to slide downwards.

It is also by the action of worms that ancient pavements, coins, weapons, &c., left upon the surface of the earth, gradually disappear from view. Mr. Darwin mentions that a footway leading across his lawn, and constructed of flagstones set edgewise, had become covered with an inch of mould between the years 1843 and 1877.

The interesting account of the habits of the earth-worm can be but very briefly noticed. From careful observations and experiments it appears that worms do not possess eyes, but can nevertheless distinguish light from darkness. If brightly illuminated they withdraw, not by a reflex action, but voluntarily. They have no power of hearing, but are very sensitive to vibrations and to changes of temperature. They appear to possess the senses of smell and taste, though very imperfectly developed. Though omnivorous, and even given to cannibalism, they prefer some kinds of food to others. From the experiments of Mr. Darwin, on the manner in which they drag into their burrows leaves of different shapes, they may even be considered as not entirely without intelligence.

We must here close our survey of this work, with which naturalists will do well to make themselves acquainted, both for the facts which it makes known and as a model how researches of this nature ought to be conducted.

On Miracles and Modern Spiritualism. Three Essays. By ALFRED RUSSEL WALLACE. Second Edition. London: Trübner and Co.

THE phenomena commonly known as spiritualistic occupy a most exceptional position. If a man of Science, of well-proved merit, dissatisfied with the vulgar explanations of "jugglery and imposture," ventures to examine the question as he would any other unsolved problem, he is forthwith assailed with the most scandalous misrepresentations. If a reviewer in dealing with a Spiritualist work expounds its tenets fairly and candidly, he too has sinned against those self-constituted authorities who presume to dictate what we are to investigate and what we are to overlook. As perfectly disinterested spectators we cannot but suspect that this evident wish to suppress is the outcome of a fear lest some, at least, of the teachings of Spiritualism should be true. Every man of Science in the present century is—or at least professes himself—ready to submit his most cherished theories to revision. Is then Monism—the doctrine that there exists in this universe naught save matter and motion—so dear to some of us that any testimony which might possibly tell against it must be dismissed unheard? We fear this is in some quarters the prevailing sentiment.

The work before us is one which may demand the serious and respectful attention of the scientific world. Its author is no weak-minded, ignorant fanatic. As the independent co-originator with Darwin of the doctrine of Natural Selection, as the author of "The Malay Archipelago" and the "Geographical Distribution of Animals," he has earned a world-wide reputation. He is everywhere recognised as a careful and accurate observer of facts, and no less as a happy generaliser. His suggestiveness, his power of explaining difficulties, are well known in the scientific world. His early training, as he himself points out in his Preface, was of such a nature that he became a "thorough and confirmed materialist." Further, it cannot be assumed that Mr. Wallace, by coming forward as a believer in Spiritualism had anything to gain. On the contrary, by the part that he has taken he has in some quarters decidedly damaged his reputation. He himself gives us the reason for the striking, though gradual, change in his opinions:—"The facts beat me." His curiosity was excited; his desire for knowledge and love of truth led him on. He strove vainly to account for the phenomena on the known principles of modern Science, and at last by slow degrees he felt compelled to accept the spiritual explanation. Surely after such avowals, coming from such a man, Spiritualism demands a more heedful examination than has been accorded to it by unscrupulous egotists and obscure "exposers," craving for notoriety if even under an *alias*.

A very important question will here suggest itself to biologists —Are the doctrines of Spiritualism compatible with the theory of Evolution? Mr. Wallace upholds the affirmative in a passage which we must take the liberty of quoting in full. He says:— "Having, as above indicated, been led by a strict induction from facts, to a belief,—1stly, in the existence of a number of preter-human intelligences of various grades; and 2ndly, that some of these intelligences, although usually invisible and intangible to us, can and do act on matter and do influence our minds,—I am surely following a strictly logical and scientific course in seeing how far this doctrine will enable us to account for some of those residual phenomena which Natural Selection alone will not explain. In the 10th chapter of my "Contributions to the Theory of Natural Selection" I have pointed out what I consider to be some of these residual phenomena, and I have suggested that they may be due to the action of some of the various intelligences above referred to. This view was, however, put forward with hesitation, and I myself suggested difficulties in the way of its acceptance; but I maintained, and still maintain, that it is one which is logically tenable, and is in no way inconsistent with a thorough acceptance of the grand doctrine of Evolution, through Natural Selection, though implying (as indeed many of the chief supporters of the doctrine admit) that it is not the all-powerful, all-sufficient, and only cause of the development of organic forms." We must venture to suggest to Mr. Wallace the further expansion of this interesting idea.

This reference to the compatibility of Spiritualism with the theory of Evolution reminds us of another point. At Spiritualist *séances* the forms manifested to the spectators are (or are supposed to be) the spirits of human beings exclusively. Now in conversation with Spiritualists we have more than once thrown out a friendly challenge that they should seek to obtain manifestations of pre-human anthropoids, missing links, and other extinct animals. Not being, for the most part, naturalists, they have paid no attention to our suggestion, or have perhaps regarded it as a mere joke. But we maintain that such manifestations would, equally well with the appearance of deceased men and women, prove the existence in living beings of an element not destroyed by death. Further, the ordinary spirit manifestations are open to the objection that the medium or a confederate artfully personates the supposed shade. But it would be impossible for such persons to personate successfully a Pterodactylus, a Hyenodon, or a Pythonomorpha. To naturalists such manifestations would be priceless; they would supply absolute demonstration of the theory of Evolution, and prove the continuity of mankind with the lower animals. Will Spiritualists make the attempt? Having put forth this suggestion it was with much pleasure that we read some remarks by Mr. Gerald Massey ("Light," Oct. 15) to this effect:—"It would be of equal inte-

rest to the evolutionists to know that the Spirit of a monkey persisted (habits and all) as if it had been the Spirit of a man, and it would give me just as much pleasure to learn that our poor relations do continue as if I received a message from some far more highly developed being."

Mr. Wallace's book consists of three distinct treatises:—"An Answer to the Arguments of Hume, Lecky, and others, against Miracles," "The Scientific Aspect of the Supernatural," and "A Defence of Modern Spiritualism."

The first of these essays, which was read eleven years ago before the Dialectical Society, and was printed for private circulation, is, in our opinion, a most complete refutation of Hume's argument. The great Scottish philosopher gives in the outset two definitions of miracles. He says that "a miracle is a violation of the laws of Nature," and again, "A miracle is a transgression of a law of Nature by a particular volition of the Deity, or by the interposition of some invisible agent." Mr. Wallace shows that these definitions are both bad, and that they in fact beg the question by the improper use of the terms "violation" and "transgression." He writes "the first definition assumes that we know all the laws of Nature; that the particular effect could not be produced by some unknown law of Nature overcoming the law we do know: it assumes, also, that if an invisible intelligent being held an apple suspended in the air, that act would violate the law of gravity. The second definition is not precise; it should be 'some invisible intelligent agent,' otherwise the action of galvanism or electricity when these agents were first discovered, and before they were ascertained to form part of the order of Nature, would answer accurately to this definition of a miracle." Mr. Wallace proposes instead the following:—A miracle is "any act or event necessarily implying the existence and agency of superhuman intelligence." He shows further that Hume deliberately and absolutely contradicts himself as to the amount and quality of the testimony in favour of miracles; and he points out the palpable fallacy of the argument, absolutely puerile, that "miracles connected with different religions destroy each other." Mr. Wallace next considers the modern argument, that if any number of men assert that they saw the stone lion on the top of Northumberland House come down to drink at the fountains in Trafalgar Square we should not believe them. Such arguments turn upon the assumed, but unprovable, proposition that "a large number of independent, honest, sane, and sensible witnesses can separately and repeatedly testify to a plain matter of fact which never happened at all."

The arguments of Mr. Lecky and Mr. Tylor are dealt with no less ably.

The second treatise is on the "Scientific Aspect of Spiritualism." One of its most important chapters consists of notes

of the author's own observations—his personal evidence. Some of the phenomena described are exceedingly remarkable, and the explanations proposed by physiologists utterly fail. We commend this portion especially to the attention of our readers. It is interesting to trace the opinions of a confirmed sceptic reluctantly, as it were, admitting first the facts, and then their explanation. On the other hand, it is difficult to conceive of such a man, in such a frame of mind, being imposed upon, either by his own feelings -which were working in the opposite direction—or by jugglery. We regret that we are unable to devote more space to this interesting volume.

Introductory Text-Book of Physical Geography. By the late DAVID PAGE, LL.D., F.G.S. Tenth Edition, revised and enlarged by CHARLES LAPWORTH, F.G.S., &c. Edinburgh and London: W. Blackwood and Sons.

WE were of opinion that the term "physical geography" had been authoritatively condemned by "South Kensington," and its use interdicted for the future under appropriate penalties. We find, however, Dr. Page and his successor and editor still declining to lay it aside in favour of the fire-new word "physiography." As we have often had occasion to remark, when a work has passed through many editions the critic finds much of his task done to his hand. The author's definition of physical geography, as distinct from descriptive or general geography, is very satisfactory. These two phases of earth-lore have, however, much in common. Both require, as their starting-point, the notion of "quarters of the globe," which in political geography works—in England at least—so much mischief. It is not too much to say that the unity of the British empire would have been much more assured, and the resources of the colonies better developed, if the latter were described immediately after the home kingdoms, instead of being slightly glanced at in different parts of the text-books.

To return from this digression, we find, as the principal features of the edition before us, a summary of the main results of the *Challenger* Expedition, an account of British storms, a description of the biological regions of the earth, and a short sketch of Prof. Huxley's arrangement of the human races. Of this added matter the last-mentioned, or ethnological portion, is perhaps the least satisfactory. Thus the "Caucasian" race of Blumenbach is an ill-assorted group as confounding together the two very distinct and antipathetic groups of nations, the Aryan or Indo-germanic and the Semitic or Aramean. We must demur

to the statement that "the Caucasian is taller than any of the other varieties." The aborigines of the most southern part of America, the Maories and the Caffers,—all, be it remarked, dwellers in the Southern Hemisphere,—are at least equal, if not superior, in stature, to any nations of the white race. We must also question the assertion that "Wherever the white man has established himself the other races disappear before him." In North America the red men, and in Australia the "black fellows," have thus faded away on coming in contact with European settlers. But recent statistics make known the alarming fact that the negro and negroid population is increasing more rapidly in America than the white inhabitants of European descent.

It is further at least questionable to include the Hottentots under the Ethiopian race, and to class together the aborigines of Australia with the Maories and the Polynesians. Mr. A. R. Wallace has pointed out the broad distinction between the Malay and the Papuan. The classification of Dr. Huxley, of which an abstract is given, avoids some of these difficulties.

The work generally is to be commended as giving not a mere description or catalogue of facts, but as tracing out their connections and causes. It appeals not so much to the memory of the student as to his reason.

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in the Victoria University, Owens College, Manchester. Vol. III.—The Chemistry of the Hydrocarbons and their Derivatives, or Organic Chemistry. Part I. London: Macmillan and Co.

The great work of Profs. Roscoe and Schorlemmer has, in its earlier volumes, deservedly won the favourable opinion of the scientific public. It possesses certain distinctive features which secure for it a wide appreciation even as compared with such formidable rivals as the great "Dictionary" of Watts and the well-known "Elements" of Miller. Without launching out into the boundless region of chemical technology, the work before us seems to bridge the gulf which separates theory from practical application in a peculiarly happy manner. Its illustrations, not merely of laboratory apparatus, but of industrial plant, are remarkable for their excellence and accuracy. To see how well this character is maintained in the present volume we need merely refer to the account of practical distillation and to the chapter on alcohol.

The volume begins with an ably-written historical introduction,

in which the early history of organic chemistry is sketched, with especial reference to the gradual changes in theory. Here we notice a curious clerical or typographical error: we read "The early ideas of Van Helmholt and afterwards of Stahl." The authors evidently mean to say "Van Helmont." After discussing the definitions of organic chemistry proposed in the earlier part of the century, and remarking that "at the present day the belief in a special vital force has ceased to encumber scientific progress," the authors define this portion of their subject as "the chemistry of the hydrocarbons and their derivatives,"—a more precise notion than, as was once proposed, the "chemistry of the carbon compounds."

We next come to an elaborate and thoroughly illustrated chapter on ultimate organic analysis, to which is appended an account of the various methods of determining vapour-densities, as adopted by Dumas, Gay-Lussac, Hofmann, and Victor Meyer. In the section on empirical and rational formulæ we meet with the profoundly philosophical remark, ascribed to Kekulé, that "the true aim of chemistry is not so much the study of the existing substance as that of its past history and its future development." This point of view in one sense approximates chemistry to biology, as studied in the light of the doctrine of Evolution.

After discussing isomerism, metamerism, and polymerism, the authors proceed to classify the carbon compounds under four great heads:—the fatty group; a nameless group of unsaturated compounds, containing relatively less hydrogen than the above; the aromatic group; and a mob of compounds of unknown constitution which the progress of research is daily reducing in number. Into the description of the bodies belonging to these classes there is the less need for us to enter, as wherever we have turned we recognise the accuracy which the eminence of the authors warrants us in expecting.

Concerning formic acid we may remark that the occurrence in the animal world is far more common than chemists seem to have recognised. It not impossibly stands in a close relation with the poisonous secretions of scorpions, centipedes, spiders, &c. Its great value as an antiseptic (referred to on p. 274) is a point which has been overlooked by sanitary reformers. As being at once more powerful than phenol, and free from the unpleasant odour of the latter, its industrial preparation on the large scale ought to attract the notice of chemical manufacturers.

The occurrence of common or ethylic alcohol in Nature—some cases of which are here mentioned—will be for many readers an interesting, and for others an unwelcome, fact. Concerning the so-called "methylated spirit" the authors justly remark that it is "unfit for human consumption." Nevertheless, except a very strict watch is kept in establishments where this spirit is in use,

it is consumed, and that to an unpleasant extent. It is also unfortunate that, in spite of this addition, the mixed spirit cannot be bought without a further addition of shellac, unless the consumer can comply with certain onerous conditions. It need scarcely be said that, save for the varnish-maker, shellac is one of the most unfortunate "denaturising" additions that could have been selected.

Judging from the present part, we should expect that two more volumes will be required to complete the work on its present scale.

A Treatise on Comparative Embryology. By FRANCIS M. BALFOUR, LL.D., F.R.S. In Two Volumes. Vol. II. London: Macmillan and Co.

WE have here the second and concluding part of Dr. Balfour's great work, and can now form some adequate idea of the labour and research which have been required in its compilation. In the first ten chapters of the volume the author expounds the developmental history of the Chordata from the Cephalocorda up to the Mammalia. In the eleventh chapter we find a short comparative review of the formation of the germinal layers and of the early stages in the development of the Vertebrata, the author treating successively of the formation of the gastrula and the behaviour of the blastopore, with the origin of the hypoblast; the mesoblast and notochord; and lastly, the epiblast. From the last-mentioned are formed the central nervous system, and the epidermis, which also takes a main share in the formation of the organs of special sensation.

From the hypoblast are formed the epithelium of the digestive canal, of the trachea, bronchial tubes, and air-cells, the cylindrical epithelium of the ducts of the liver, pancreas, &c., and the spheroidal secreting cells of the pancreas and other glands. From the monoblast are formed the true skin, the muscles, the skeleton, the vessels, generative and urinary organs. It is particularly noted that the epithelium of the urinary glands, though resembling the hypoblastic epithelium of the alimentary canal, is nevertheless mesoblastic.

The interesting question of the growth of the vertebrate embryo is next discussed. On this subject two views prevail. The author, in common with the generality of embryologists, holds that, *e.g.*, the Elasmobranch embryo arises from a differentiation of the edge of the blastoderm, extending for some little distance inward from the edge. This differentiation is considered to comprehend the rudiments of the entire embryo, except the yolk-

sac; and its hinder end, at the edge of the blastoderm, appears to correspond with the same extremity of the mature body. Growth in length depends on a process of intersusception, and, until the full number of mesoblastic somites are formed, it is effected, as in the Chætopods, by the constant addition of somites between the one last formed and the posterior end of the body.

On the other hand, His and Rauber assert that growth in length is effected by a coalescence of the two halves of the thickened edges of the blastoderm in the median dorsal line. The arguments in support of this view are, briefly stated, the general appearance, certain measurements which to the author seem mainly to prove that the growth takes place by the addition of fresh somites between the end of the body and the last-formed somite, and, lastly, certain phenomena observed in double monsters. An examination of the growth of the embryo proves, however, that points brought forward by His and Rauber are far from conclusive, and that the growth in length of the greater part of the body certainly takes place, as in the Chætopods, by the growth of fresh somites. Hence it would be strange if a small part in the middle of the body should grow in a different way. If the vertebrate blastopore had the same extent as the dorsal surface, as His and Rauber assert, this should appear in *Amphioxus*. But in this species it is at first placed quite at the hind end of the body, and nearly closes up before the appearance of the medullary groove or the mesoblastic somites. The medullary folds have nothing to do with its lips. Again, in the Vertebrates the food-yolk is situate on the ventral side of the body, and is enfolded in the blastoderm. Hence in all "large-yolked" forms the ventral walls are completed by the lips of the blastopore closing on the ventral side.

On the "concrescence" hypothesis of His and Rauber the dorsal walls are also completed by the closing of the blastopore, so that the concrescence of its lips would form both the dorsal and the ventral, as well as the dorsal walls of the embryo. This consideration Dr. Balfour thinks amounts to a *reductio ad absurdum* of the whole theory.

In Chapter XII. the author discusses the ancestral form of the Chordata. He considers it clear that the ancestors of the Chordata were segmented, their mesoblast being divided into myotomes, which even extended into the region in front of the mouth; so that the head, as well as the trunk, was segmented. The only internal skeleton was the non-segmented notochord. Hence Dr. Balfour draws the corollary that the skeleton is of relatively little value for the decision of many fundamental questions. The region which now forms the œsophagus and stomach was in the ancestral forms severed by gill-clefts. The mouth had a suctorial character, and had its place on the ventral surface behind the pre-oral lobe In the higher types it has gradually become modified to a biting structure, and has been

removed to the front of the head. The alimentary canal probably extended to the end of the tail, and the present anus in the Vertebrates is therefore not the original one, but is a formation not inherited from any older group, but acquired within the group of Chordata. This transference of the anus is not easily accounted for on the Darwinian theory.

At the end of this chapter we have a tabular scheme of the phylogeny of the Chordata. We find a series of hypothetical groups—the Protochordata, from which are supposed to spring the degenerate existing groups of Urochorda and Cephalocorda, and the hypothetical Proto-vertebrata. From these are descended a degenerate type, the Cyclostomata, and another hypothetical group, the Proto-gnathostomata. These give rise to the actual Elasmobranchii and Holocephali, and the hypothetical Proto-ganoids, from which spring the existing Dipnoi, Ganoids, and Teleostei, and the hypothetical type Proto-pentadactyloids. These are the ancestors of the real amphibians, and of a final hypothetical group, the Proto-amniota, the progenitors of the Sauropsida (birds and reptiles), and of the mammals. It is considered probable that the mesozoic Plesiosauri and Ichthyosauri were more closely connected with the Proto-pentadactyloids than either to the Amphibia or the Prota-amniota.

The thirteenth chapter is devoted to an examination of the origin and homologies of the germinal layers, and to the consideration of the nature, origin, and affinities of larval forms. Here, from the difficulty of the subject, and often from the absence of the necessary researches, the author aims more at summarising and criticising the views of others than at coming to any final decision. He considers that the gastrula reproduces, more or less closely, a stage in the development of the Metazoa which is permanent in the simpler Hydrozoa when the organism possesses a fully developed digestive cavity lined by the hypoblast and endowed with assimilative powers. An oral aperture and an epiblast were in existence. The following weighty questions, however, remain, as the author admits, unsolved:—"By what steps did the compound Protozoon become differentiated into a Metazoon? Are there any grounds for thinking that there is more than one line along which the Metazoa have become independently evolved from the Protozoa? And to what extent is there throughout the Metazoa a perfect homology between the two main germinal layers?"

The section on larval forms begins with a comparison of larval and fœtal development. Whilst examining this chapter we could not help regretting that so few embryologists are entomologists, or so few entomologists are embryologists.

The remaining eleven chapters of the work, which want of space does not allow us to summarise, are devoted to organogeny. The author treats in succession of the epidermis and its derivatives; of the nervous system; the organs of vision, hearing,

scent; the indefinite sense organs of the median line; of the notochord, vertebral column, ribs, and sternum; the skull, pectoral and pelvic girdles and limbs; the body cavity and vascular system; the muscular system; the excretory and generative organs and the alimentary canal in the Chordata, with its appendages. It seems to us that in this survey the insects fare worse than any other great department of the animal kingdom, not from any intention on the part of the author, but from the frequent want of the necessary materials.

The work is abundantly and well illustrated, and every division is furnished with an elaborate bibliography. As to the value and utility of the book, we must admit it to be on its subject without a worthy parallel in the language. It is equally suitable as a manual for the student, and as a work of reference for biologists of old standing.

Inorganic Chemistry, Theoretical and Practical. An Elementary Text-Book designed primarily for Students of Science-Classes connected with the Science and Art Department of the Committee of Council on Education. By W. Jago, F.C.S., A.I.C. London: Longmans and Co.

We should much like to see an accurate return of the number of elementary text-books on chemistry which have appeared within the last dozen years, all of them, or nearly so, compiled with a special reference to the examinations of the Science and Art Department. How it comes that there is scope for so many works covering the same ground, and how publishers are prevailed upon to bring them out, are unsolved problems. It would be a very different case if there were in England, as in Germany, and to some extent even in France, a number of eminent and independent schools of chemistry, each possessing some distinctive feature. But with us South Kensington reigns supreme, and lays before every student a *quicunque vult* from which there is no dispensation. In consequence all our text-books and manuals bear the same stamp.

To do him justice, Mr. Jago feels the difficulty of the situation. He begins his Preface with certain remarks so sound and judicious that we cannot help quoting them at some length:—"The teacher of classes which are in connection with the Science and Art Department has to face special difficulties. In addition to his desire to give his students a thorough knowledge of their subject, he must necessarily remember that it is all-important to him that good results be obtained at the Department's examinations. Unless, therefore, he is prepared to sacrifice the true

teaching of the subject altogether, he must perforce keep in view two ends that are distinct from each other. The writer of a special text-book such as this encounters the same difficulty; there is, on the one hand, the strong temptation to treat the subject so as to give a sound elementary basis of knowledge, regardless of examiners and examinations; or, on the other, to study the idiosyncracies of particular examiners, and simply to provide the material for answers to their questions."

We have seldom seen the case against the Science and Art Department put so tersely, so calmly, and yet with such overwhelming power. Here is the admission that a "thorough knowledge"—say, of chemistry—does not necessarily imply success at the examinations; that the latter object is something perfectly distinct from the former; and that teacher and pupil are both encumbered by attemping to combine two incongruous aims. It is further confessed that "to sacrifice the true teaching of the subject" is a way, and an easier way, to good results at the examinations. It is further plain that examination no longer seeks to play the humble though perhaps necessary part of a test of the student's knowledge; it is made not a means, but an end. Hence all teachers who do not indulge in the costly luxury of a conscience do actually sacrifice true teaching to getting a large proportion of their students to pass. Hence those pseudo-students who have no love for Science, and who seek merely certificates, medals, and degrees, are enabled to thrust sterling merit into the background.

The "Science and Art Department" is one of the saddest mistakes into which a nation ever fell; and if we wish to make sound progress in Science, pure or applied, one of our first steps should be its total abolition.

Technical Vocabulary, English-French, for Scientific, Technical, and Industrial Students. By Dr. F. J. Wershoven, Author of the English and German and French and German Technical Vocabularies. London and Paris: Librairie Hachette et Cie. Boston: Karl Schœnhof.

This useful little work is divided into three sections. The first gives, in parallel columns, the French and English technical terms used in mechanics and in all the branches of physics; the second section treats, in the same style, of chemistry and metallurgy; whilst the third is devoted to machinery, railways, and to certain arts and manufactures. Astronomy and biology, with the applications of the former in navigation, and of the latter in medicine, agriculture, &c., are not included.

The general character of the work is thorough accuracy, the more to be noted as neither French nor English is the author's native language. Yet with scarcely an exception his English is that of an Englishman, and his French, as far as we can judge, that of a Frenchman, well acquainted with his subject. The value of the work for Englishmen who wish to make themselves acquainted with the technical literature of France is indisputable.

Elementary Treatise on Physics, Experimental and Applied. For the Use of Colleges and Schools. Translated and Edited from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D., F.C.S. Tenth edition, revised and enlarged. London: Longmans and Co.

The editor and translator of this work is guilty of no departure from the truth when he refers to the "continued and increasing favour" which it experiences both as a college manual and as a work of reference. The subject-matter has been brought to a level with the present state of knowledge. Thus the researches of Cailletet and Pictet on the condensation of oxygen, &c., are now duly noticed, and the category of permanent gases has consequently disappeared. The recently invented electric-acoustic instruments, such as the photophone, are duly described and figured. The electric light and the various systems for its production are spoken of at some length, and the interesting experiments of Siemens on its effects upon vegetation are described. Paragraphs are devoted to the experiments of De la Rue and Miller on the stratification of the electric light, and to the phenomena observed by Crookes on producing the electric discharge in tubes very highly exhausted. The theoretical views which Mr. Crookes has deduced from his results are referred to merely as being still *sub judice*.

The subject of polarised light, which if our memory serves us was to some extent overlooked in former editions, now receives due attention.

In estimating the value of this work we must take into account the translator's reservation that it does not and cannot attempt to give an exhaustive account of any one branch of Physics, since such an undertaking would be inconsistent with the purpose of a college text-book, and in so voluminous and rapidly-growing a science would be impracticable even within a fourfold larger compass. What the author and translator have aimed at is ably, accurately, and clearly done, and we can have no hesitation in giving the work as it now stands our cordial recommendation. There is a very complete index, the references being not to the pages, but to paragraphs.

Evenings at Home in Spiritual Séance. Prefaced and Welded together by a Species of Autobiography. By Miss Houghton. First Series. London: Trübner and Co.

Never in our experience have we met with a more perplexing book than the one before us. It would of course be exceedingly easy for some critics to turn the author and her narrative into ridicule, and to dismiss her as an impostor or a lunatic. Unfortunately for ourselves we cannot dismiss difficult problems so lightly. Whoever reads these "Evenings at Home" in a candid spirit must at least admit that Miss Houghton writes in perfect faith, fully believing every statement which she has advanced. But are the sights, the sounds, which she has experienced and recorded, objective realities or merely illusions? Here is the difficulty. Miss Houghton may be of an enthusiastic, imaginative temperament, and may possibly have mistaken subjective imagery for external objects. We do not say this dogmatically, but doubtingly, and well aware of the exceeding imperfection of human knowledge concerning all those phenomena which are classed as mental. Let us call attention to certain points: the lady in question is, if not an artist by profession, evidently an admirer of art, acquainted with living artists, and not unfamiliar with the names and the peculiarities of the great masters of the past. Accordingly she becomes a drawing medium; she is brought into communication with the spirits of Sir Joshua Reynolds, Salvator Rosa, Claude Lorraine, Parmegiano, Vandyck, &c. Again: she is evidently a devout Christian, well read in religious literature in general, and especially in the Bible. We find her brought in communication with the archangels Gabriel and Michael, with the spirits of Abraham, Moses, Aaron, Luther, Elias, &c. We cannot help asking whether the same forms would have appeared to Miss Houghton had she been brought up among different surroundings and different traditions? We know, of course, that to this objection a reply may be made. It may be said that artist-spirits were by a natural affinity attracted to the living artists, and saints and angels to the devout believer.

Perhaps the most satisfactory proceeding will be to quote the author's account of the evidence which originally made her a Spiritualist, so that our readers may judge for themselves concerning its validity. In the year 1859 she was told by her cousin, Mrs. Pearson, that there were means by which it was possible to communicate with the spirits of lost friends. The two ladies made, accordingly, a visit to Mrs. Marshall, who with her niece "sat with us at a round table. In a short time the raps came." . . . "My youngest sister, Zilla," who died in 1851," was again in conversation with me, and I asked whether anything still troubled her, to which she answered 'Yes,' and on my enquiring what it was, naturally concluding that it would refer to her

husband or children, I was surprised by the word 'Helen' being spelt out. Helen was another sister, three years older than herself, who had married a Catholic, and had been led away to the same church. This had troubled Zilla on earth, and I felt it was still a grief to her." Miss Houghton comments on this occurrence as follows:—"The name of Helen could never have been the coinage of Mrs. Marshall or her niece, nor was she at all in my thoughts, but I immediately understood what Zilla meant." The question then naturally arises, How is this to be accounted for? Supposing, for argument's sake, that Mrs. Marshall and her niece were jugglers, what should lead them to conclude that Zilla would be thinking of a sister rather than of her husband and children? It is not indeed expressly stated, but we should judge from the passage that Mrs. Marshall had no previous acquaintance with Miss Houghton and her family. We read further "Later in the evening the planchette was brought forward, and on it were placed Mary Brodie's hand (the niece of Mrs. Marshall) and my cousin's (Mrs. Pearson). I then asked her how many years it was since the first brother I had lost had passed into spirit-life? A 3 was written, and my cousin (who was thinking of another brother) said, 'It is going to be 13.' Instead of which another 3 was added, which was correct, for it was thirty-three years since I lost my dear brother Cecil Angelo."

These two tests were for Miss Houghton sufficient. But it is right to add that very singular, even startling, occurrences are narrated in the course of the book which are exceedingly difficult to explain on any other supposition than either wilful deceit or the intervention of intelligent beings other than men and women as we meet with them in life. Some of the cases are of such a nature that accidental coincidence is scarcely thinkable. We quote as a subject for reflection the following remark:—"I think most of my readers will be able to remember some occasion when a thought will seem to have flashed into their minds without any connection with their then train of ideas, and they may feel assured that it has been thus deposited in their brain by some external agent, and probably for a specific purpose."

The author learnt from a Mr. Hyde that the celebrated Sir H. Bessemer in the early part of his career saw, in a dream, the way to overcome a practical difficulty in connection with a type-distributing-machine which he was then engaged with. She ascribes this to the intervention of spirits—an explanation which we do not think necessary. We have learnt, however, that a gentleman, who has made some signal improvements in connection with the alkali manufacture, considers that he has been assisted by spiritual agency.

The following is a curious statement: "One Sunday in 1862, soon after the clergyman had gone into the pulpit, I saw him irradiated with a violet light like a globe of about 2 feet in diameter, the central point of which appeared to be placed at the

heart. A Sunday or two later another clergyman preached, when again a violet sphere was shown me, but this was larger, at least 3 feet in diameter. On the next occasion a stranger occupied the pulpit, and he was in like manner surrounded by a delicate green light."

In connection with this we venture to put the following: "She (Mrs. Hallett) afterwards saw a beautiful spirit-light, which seemed to descend from the north-west and gradually spread out as if on all sides. It was to me very striking that light should come from the north-west, for in all my drawings that has been the point of glory." Have we here some luminous phenomenon accompanying or connected with terrestrial magnetism, and under conditions yet unknown visible to man? Are all organic beings enveloped in globes of coloured light?

The author, as well as a Mr. Spear often here mentioned, speaks of numbers as good or bad. Every multiplicand or dividend of twelve is good!

Miss Houghton is convinced of "the spiritual sight of animals, and their after life." But the instance which she gives in support of the former view, and which we cannot quote from want of space, is by no means conclusive.

A strange story, given on pp. 63 and 64, is that of Mumler, the Boston photographer, who, taking a photograph of himself one day when perfectly alone, found on developing the picture, the image of a second person beside his own, and recognised it as that of a cousin, who had been dead about twelve years! All here turns on an unknown quantity—the truthfulness of Mumler.

Those of our readers who are able honestly and fairly to suspend judgment in doubtful cases, and who are not beset with "DOMINANT IDEAS," will find this book worth carefully reading.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

DARKNESS IN MASSACHUSETTS.

To the Editor of the Journal of Science.

Sir,—About a fortnight since the telegrams from America stated that almost complete darkness was experienced during the day in Massachusetts. I have not seen any further account of it in the London papers. Have you seen any explanation of this phenomenon?

I find, in the "History of Lynn, Massachusetts," under the "Chronological Table," the following:—

1716. Extraordinary darkness at noonday, October 21st: dinner tables lighted.

1780. Memorable dark day, May 19th: houses lighted as at night.

—I am, &c.,

Charles W. Harding.

INSECTS DURING THE PAST SEASON.

To the Editor of The Journal of Science.

Sir,—Perhaps the following notes in illustration of the influence of seasonal peculiarities upon animal life may be of interest to some of your readers. It will be remembered that the spring and early summer of 1881 were uncommonly dry, cold, and bleak; that we had a short fit of unusual heat in July; and that since the end of the first week in August the temperature has been low, rain abundant, and high winds plentiful.

A prominent feature has been the general rarity of wasps: bee-keepers, and growers of grapes, peaches, &c., under glass, report very unanimously that they have had no trouble with these marauders throughout the season. A few have appeared in sheltered places at the end of September and the beginning of October. Hornets have been still scarcer. In the neighbourhood of Chingford, Sewardstone, &c., where they are usually common, none have been seen.

The cockchaffer and his diminutive successor, *Rhisotrogus solstitialis*, have both been scarce, and have occasioned no damage.

The currant-moth (*Abraxas grossulariata*) has not been plentiful. Gooseberry and currant bushes, which were much infested with its larvæ in 1879 and 1880, have been this year nearly free. The yellow underwing (*Tryphæna pronuba*) was very plentiful, but only for a short time. *Orgyia antiqua* has been very plentiful in the suburbs of London and the country districts around, and has done much mischief.

Earwigs have been abundant almost beyond precedent during the whole season. Lettuces in May, roses and carnations in the summer months, and dahlias and asters in September and October, have been full of these pests.

The crane-flies have been scarce, but gnats and biting flies (*Stomoxys*) very abundant. Dragonflies have been unusually scarce.

In connection with the multitudes of gnats, I may mention the scarcity of swallows. I know an instance of a mansion where a few years ago they were very abundant, and where they were carefully protected by the proprietor. This year there have been scarcely any.—I am, &c.,

S.

NOTES.

In a paper read before the Entomological Society, by Mr. R. McLachlan, F.R.S., a decided case of parthenogenesis among Coleoptera was recorded. A virgin female *Gastrophysa raphani* deposited several successive batches of eggs at intervals of several days, and in each batch one egg at least proved fruitful.

Dr. Rush, of Philadelphia, gives ("Science") an interesting case of regressive recollection of languages. Dr. Scandella, a learned Italian, residing in America, had a complete mastery of French and English, as well as of his native tongue. Being attacked with yellow fever, in the beginning of his illness he spoke English only. As he became worse he spoke nothing but French, and on the day of his death Italian only.

The "Social Science Congress" is, even more than the British Association, in danger of becoming a political debating society. It is doing everything but constituting sociology.

According to Mr. A. Strahan ("Geological Magazine") the coal found beneath the New Red Sandstone, in the south-west of Lancashire, is for the present not likely to be of practical importance.

Prof. Barrett ("Psychological Review") questions Dr. Carpenter's—or we should rather say Dr. Beard's—explanation of "thought-reading."

The "Scottish Naturalist" gives an obituary notice of the late Robert Walker, F.G.S., Registrar of the University of St. Andrews, a zealous and successful observer in geology and marine geology.

Prof. Seeley, F.R.S. ("Geological Magazine"), after a careful comparative examination, maintains that the London and the Berlin specimens of *Archæopteryx* belong respectively to different species.

The Rev. F. O. Morris writes to the "Morning Post" that a specimen of the Californian quail has been recently shot near Huggatt, a village on the Yorkshire Wolds.

According to M. Ravaisson ("Comptes Rendus") the unpublished manuscripts of Leonardo da Vinci contain suggestions for aërial locomotion, a description and diagrams of a steam-cannon, and an account of a device for hearing distant sounds, either on land or sea. (It must be noted that, like certain inventors of the present day, Da Vinci turned his attention chiefly to appliances for warlike purposes.)

Prof. Peirce, in a publication bearing the very curious title "Sketches and Reminiscences of the Radical Club," maintains that if meteors did not fall into the sun the temperature of the earth would sink to $-200°$ or $-300°$.

Dr. F. Buchanan White, F.L.S., gives, in the "Scottish Naturalist," an interesting account of the habits of *Dytiscus lapponicus*, a large carnivorous water-beetle, occurring in Britain only in the Isle of Mull, in Strathglass, and in Donegal.

Mr. E. Wethered, F.G.S., in a memoir on the "Formation of Coal" read before the British Association, contended that coal was not formed from trees of the Lepidendroid type; that the *stigmaria* found in the under clays are not the roots of the vegetation which gave rise to the coal; and that the varieties of coal are not due to metamorphism, but depend on the different degree of decomposition of the vegetable mass when submerged. He argues that coal must have been formed from a compact mass of vegetation, and could not have arisen from large trees growing *in situ*.

Mr. T. R Jones ("Geological Magazine") states that the horns of *Cervus Megaceros* obtained in Berkshire were found in the peat itself, and not in the underlying clays.

Dr. E. C. Spitzka ("Science") found in the egg of a turtle (*Chrysemys picta*), laid in his experimental tank, "a live maggot, the larva probably of *Musca vomitoria*, crawling in the space between the half-dried yolk and the shell membrane." He observes that foreign bodies have frequently been found in hen's eggs, as legs of beetles, straw, &c.; but he believes there is no case on record of a living animal occurring in an egg. (We once found in a hen's egg a small piece of printed paper.)

The "Medical Press and Circular" is ably pointing out the permanent and serious injuries which often result from bodily punishments as administered in schools.

Dr. Sharp has discovered, in the Sandwich Islands, thirty-four species of insects new to Science.

Mr. "Stuart Cumberland" has been enlightening the Church Congress on the subject of Spiritualism.

Two curious facts appear in the writings of the so-called Theosophists: the re-habilitation of the ancient elements, fire, air, earth, and water; and the inculcation of asceticism as the road to superior wisdom and power.

The election of a Lord Rector is about to take place at the University of Aberdeen, and we much regret that the contest will probably be fought on political grounds.

Two species of *Phasma* are occasioning great damage among the cocoa-nut plantations of the Fiji Islands.

The resolution in favour of vivisection unanimously adopted at the late International Medical Congress leaves something to be desired. It asserts merely "That this Congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable for its future progress; that, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion that, in the interest of men and animals, it is not desirable to restrict competent persons in the performance of such experiments." It seems to us that the resolution covers only experiments made with a distinctly medical purpose. The word "competent" is, further, invidious. We regret the want of union between the medical profession and non-medical biologists on this important subject.

Mr. Harley Barnes ("American Naturalist"), whilst denying that animals possess reason, neatly refutes himself by admitting that they "reason from cause to effect."

Prof. W. J. Beal, addressing students of botany, most wisely says "Beginners should study plants and refer to books, and not study books and refer to plants."

An Italian, upon whom adjectives would be wasted, has lately been boasting of the numbers of swallows which he has shot. We should like to have him stripped naked, and tied to a tree in a certain district in the Lower Danube Valley. Before the next morning the mosquitoes and sandflies would have convinced him of the value of bird-life.

The blackbird is now recognised as having an occasionally carnivorous and predatory character.

Prof. Von Pettenkofer, at the recent meeting of the German Congress of Naturalists and Physicians, maintained that the sanitary condition of the soil was of the highest moment, whilst the air and the waters, if polluted, soon purified themselves again.

The larva of *Crambus vulgivagellus*, a small moth belonging to the Pyralidæ, has this year destroyed the grass over many hundreds of acres in the State of New York.

The Hon. W. Bross, in a paper read before the American Association for the Advancement of Science, ascribes the origin of canons not to the erosive action of rivers, but to "some great convulsion of the earth's surface, or the contraction of mountain chains from their igneous condition in the early history of the planet."

Prof. Owen ("Journal of British Dental Association") maintains that the alleged cases of a third set of teeth are merely the reappearance of old and worn stumps, in consequence of the shrinkage and absorption of the jaw.

With reference to the hay- and corn-drying machine described in our October issue, we may mention that a contemporary estimates the loss sustained in the United Kingdom, from the bad harvests of 1879 and of the current year, at £150,000,000.

French papers report that a dog at Metz has—not from its own folly, but by being shut up—gone through a fast of thirty-nine days in duration. He has recovered.

The names of the Committee for the forthcoming Electrical Exhibition at the Crystal Palace are duly announced. Sir H. Cole is of course included, but why, oh why, is Mr. Buckmaster left out in the cold?

The fifty-fourth meeting of the Congress of German Naturalists and Physicians took place this year at Salzburg, from the 18th to the 24th of September, and was complicated with an Exhibition.

A Sanitary Congress was held at Vienna on the 14th of September and following days. M. Knauff, of Berlin, argued strongly in favour of a double system of sewerage, the rain-water, &c., not being allowed to mix with and dilute the sewage properly so called.

According to Dr. Mylius ("Pharm. Central Halle") only 0·001 grm. oxide of copper was extracted from the intestines of a girl poisoned with "Schweinfurt green," although 130 grms. of the liver, 60 grms. of the kidneys, and half the stomach and its contents were operated upon.

Lycopodium complanatum, L., is stated by Bödeker to contain more alumina than any other plant.

We regret having to notice the death of Prof. Schützenberger, of Strasbourg.

Sir Joseph Hooker considers that the flora of the Southern temperate zone has been derived from the Northern temperate regions.

According to Oscar Löw ("Berichte Deutsch. Chem. Gesell.") he has demonstrated the existence of free fluorine in the fluor-spar of Wölsendorf.

W. B. Schmidt ("Mineralog. Mittheilungen") points out that sulphurous acid is rare among the products of Vesuvius, and of the volcanoes of equatorial America; more common in Etna; and abundant in the Lipari, the volcanoes of Iceland, and especially in those of Java.

M. Ziegler, of Geneva, announces two magnetised iron bars, joined together in a certain manner, produce definite effects upon animals, which differ according to the angle of intersection of the bars. Prof. C. Vogt pronounces the discovery of importance.

The "London Quarterly Review" for October contains an exceedingly able but severe critique on the writings of Dr. Maudsley. The reviewer, however, ignores those proofs of moral life in animals which the unbiassed naturalist meets with frequently.

According to the "Bibliotheque Universelle," in France the term "Darwinism" is considered practically synonymous with "atheist." Certain ecclesiastical writers assert, and even seem to believe, that Mr. Darwin's primary object was anti-Christian and anti-theistic!

M. Laségue ("Revue des Deux Mondes") gives a very eulogistic summary of the researcees of Mr. Braid in "Animal Magnetism."

At the recent Congress of German Naturalists and Physicians Geheimrath Weisman read a paper on the duration of life and the cause of death. He considers that the very various durations of life in different species is not determined alone by morphological and physiological laws, but depends on an adaptation of each species to vital conditions. The cause of death he seeks in a limitation of the reproductive power of the cells. Death is a phenomenon of adaptation, immortality having been lost by the higher animals when no longer needed. An Amœba may be destroyed, but it has no fixed term of life, and its death is not a matter of necessity.

M. E. Yung ("Comptes Rendus") has made a series of experiments on the action of poisons upon the Lamellibranchiate mollusks. Atropine produces no appreciable effect; strychnine is fatal only when placed in direct contact with the heart.

A pair of twins, Jacob and Baptiste Jocci, are now being exhibited at Vienna. They are grown together from the sixth rib downwards, and have but one abdomen and two legs. Each child thinks, speaks, sleeps, eats and drinks independently of the other. One may even be indisposed without affecting the other. —*Medical and Surgical Reporter.*

M. Robin has exhibited before the Société de Biologie a specimen of blue urine, passed by a patient suffering from interstitial nephritis. The colour is supposed due to the decomposition of indican.

Dr. G. M. Sternberg ("National Board of Health Bulletin"), from observations made on the mud in the gutters of New Orleans, concludes that pathogenic bacteria are bred not alone in the bodies of animals.

At the last meeting of the Royal Microscopical Society Mr. J. W. Stephenson exhibited a slide of diatoms mounted in a saturated solution of phosphorus in bisulphide carbon. The refractive index of this medium is about 2·10, while that of diatomaceous silex is 1·43, the visibility being greater than the same

object mounted in balsam in the ratio of 11 to 67, and greatly exceeding the visibility of the same objects mounted in air. In this latter medium they could not be examined with any advantage under the recently constructed objectives, having a numerical aperture exceeding 1·0. The practical difficulties of mounting in a solution of phosphorus are very great, owing to the inflammability of the medium, so that, good as the results are, it will probably be but seldom employed.

The new "Periscopic" eye-piece of Mr. Gundlach consists of a triple eye-lens and a double-convex field-lens, the latter being situated *within* the focal distance of the former, and a diaphragm placed in the focus of the equivalent of the two lenses. The field of the new eye-piece is larger than that of Kellner, and the image is sharply defined to the extreme edge. As the focus of the eye-piece lies behind the field-lens it is particularly suitable for micrometers, especially as the divisions are distinctly visible to the extreme edge, which is not the case with Ramsden's eye-piece.

NOTE.—Absence from London having prevented Mr. Reichenbach from seeing proof-sheets of his article in Nos. 93 and 94, he apologises for the errata therein.